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THE FACULTY OF ATTENTION

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In order to justify the designation of attention as a faculty, it must be possible to demonstrate that the degree of attention of any individual at a given time depends upon certain factors that remain constant for that individual, no matter what the particular case of attention in question may be, that is, in spite of endless variation in the specific form or content of the mental processes involved. The problem is to determine whether there are conditions of attention which remain constant in all cases of attention of a given individual.

Different individuals, under apparently the same conditions, show differences in degree of attention. The individual, himself, that is, is one of the most important conditions. Of course, for the 'individual,' we may some day be able to substitute a more definite condition or list of conditions. What this list would include is at present not known, but merely as a suggestion of the meaning of these general, individual conditions, we might surmise such things as the general cortical structure or certain features thereof; the number of association fibers; the frontal lobes[1] or some function thereof; 'mental constitution'[2] or organization; or 'intellective energy.'[3] Whatever goes into the list of conditions which make individuals necessarily different with respect to degree of attention in spite of all that can be done to make

[1] Wundt, 'Physiologische Psychologie,' 6th ed., 1908, Vol. I., 378.

[2] Binet, 'Attention et adaptation,' *Anneé psychol.*, 1899, 6, 395.

[3] Hart and Spearman, 'General Ability, Its Existence and Nature,' *Brit. Journ. of Psychol.*, 1912, 5, 70.

the conditions of attention the same for them, or to allow for unavoidable inequality in the conditions, may be called the general, individual conditions. The question of a faculty of attention is merely the question whether there are any such general conditions. For any case of attention, we may write the perfectly general formula,

$$A = f(c_1, c_2, c_3, \ldots, c_n),$$

in which A represents degree of attention in any case with any individual, and $c_1, c_2, c_3, \ldots, c_n$, conditions of attention which vary in different cases of attention of the same individual. The question now is, are we justified, as long as we are dealing with the same individual, in writing this formula as

$$A = f(k, c_1, c_2, c_3, \ldots, c_n),$$

in which k is a constant which varies with the individual, but not with different cases of attention of the same individual.

In view of the great complexity of the conditions of attention and the apparently uncontrollable nature of many of them, the problem might at first thought seem impossible of direct solution. There would be no difficulty if all the c factors could be kept constant for a number of individuals; for, with these factors constant, any differences in A for different individuals would necessarily be due to differences in a factor, k. It is of course impossible to keep all the c factors constant. Most of them can not be controlled. Many, however, may be ruled out in any particular case as of negligible importance. For example, knowledge of chemistry may be a condition of attention to some topic of chemistry, but may be regarded as not affecting the degree of attention given to a tennis ball served by one's opponent. Under the particular conditions of the method of measuring attention employed in the present work, many of the conditions of attention may be regarded as ruled out. Many more may be kept approximately constant. It may be possible, consequently, to arrange a case of attention such that if the differences between individuals are due to the more or less specific c factors, they must be due chiefly to variations in

one, or one very narrow set, of *c* factors, which one or set may be altogether different in another case of attention. Now if it is possible to arrange several such cases of attention, and if we find the same individual differences in all of the several cases, these individual differences cannot reasonably be supposed to be due to differences in the *c* factors; for the latter, if to any degree specific, should show little or no correlation, certainly not perfect correlation, and individual differences due to the *c* factors should therefore vary with the use of different *c* factors. It will be shown that the conditions of attention may be controlled experimentally in such a way that, in spite of variation in certain *c* factors, and consequent effect upon the absolute degree of attention of the subjects, the individual differences in degree of attention remain relatively the same. Evidently, then, the individual differences are due to variations in a general factor, *k*, which varies only with the individual.

To make sure that the conditions necessary for this direct demonstration of the *k* factor were obtained, it is necessary to examine carefully the four different cases of attention used in the present research. Three of the cases are those of attention to a touch, sound and light, each of moderately strong intensity, with the purpose of reacting as quickly as possible. The fourth case of attention is that which occurs in a choice reaction experiment, in which the subject reacts with his right hand, if a light already visible suddenly becomes brighter, and with his left, if it becomes less bright. If the large differences found to exist between the individuals are not due to differences in an individual constant *k*, they must be due to inequality for the different individuals in some of the *c* factors. While it is impossible to discuss in detail all the *c* factors, a careful consideration of the more obviously important ones will be sufficient to make it clear that the individual differences could not have been due to inequality in any other *c* factors than those which varied in the four different cases; and the data, showing that the individual differences remain the same in these four cases, eliminate even these factors as the cause of the individual differences.

In each of the four specific cases of attention used, it is evident that all the objective conditions could be kept constant for all individuals. This includes such factors as intensity, mode and quality of stimulus, the nature of the warning signal, and the duration of the preparatory intervals. With careful instructions, such factors as zeal, interest and purpose may be regarded as constant in all four cases, and as differing between different individuals only in so far as they are general, individual factors.

Familiarity may perhaps be regarded as constant for all subjects, since every one may be regarded as maximally familiar with touch, sound and light stimuli; but in any event, this factor is not pertinent, since results already published show that degree of attention, under the circumstances of the present measurements, does not vary with practice.[1] This conclusion is confirmed by the data presented below (in Table I.). Now, if a person shows the same degree of attention the first few times he is measured as at the end of a long series of measurements, familiarity with the conditions is evidently a negligible factor.

Marked variations in sensory sensitivity of different individuals would no doubt affect the degree of attention in any of the four cases used. I have already conducted a study of means whereby this factor may be excluded. The conclusion was reached, that by the use of reactions to a *change* in intensity, attention can be measured independently of wide variations in sensory sensitivity.[2] This method was not used in the present instance, however, since it seemed highly probable that, with moderately strong intensities of stimuli, as long as fairly normal sensitivity was secured, slight variations in sensitivity would not seriously affect the results. That this surmise was correct is proved by the results obtained which show that the effect of mode of stimulus may, without serious error, be regarded as a constant one in the subjects used.

[1] Woodrow, 'The Measurement of Attention,' *Psychol. Monog.*, 1914, Vol. XVII., 5, 122–142.

[2] *Op. cit.*, Chap. IV.

Another set of specific factors which should be considered are differences which, in spite of careful and identical instructions, may have existed in the direction of attention. In all cases, the instructions were to react as quickly as possible, and to take the fingers off the key as quickly as possible, when the stimulus occurred. It is evident, however, that considerable room for variations in attentional attitude is left by any instructions. Attention can be more or less unequaly directed to the stimulus, the reaction movement and the idea of reacting as quickly as possible. These variations probably do constitute slight and variable sources of error in the measurements made, but it is quite certain that they are of no considerable importance. I made a long series of trials, several years ago, with myself and one other subject, in which I studied the effect of directing attention on some days as much as possible towards the light used as stimulus and on other days as much as possible towards the reaction movement. In all cases, every reaction was made as quickly as possible. I could find no reliable difference in the degree of attention or even in the simple reaction time, and concluded, in agreement with Ach, that the only very fundamental distinction in reactions from the point of view of reaction time is that which divides them into those which occur with the endeavor to react as quickly as possible and those which occur without such endeavor.[1] As experimental evidence of the negligible importance of variations in direction of attention, may be cited the results presented below[2] which show that about the same individual differences in degree of attention exist with a choice reaction as with a simple reaction. Since the whole attitude in a choice reaction is different from that in a simple reaction, it seems hardly probable that the individual differences in attentional attitude would remain the same in both cases.

Preparedness of motor centers for action, which some writers, in spite of severe criticism,[3] emphasize so strongly as

[1] *Op. cit.*, 21.

[2] See Table VI.

[3] Moore, T. V., 'A Study in Reaction Time and Movement,' *Psych. Rev. Monog. Sup.*, 1904, Vol. VI., 1, 62–72. Pillsbury, 'The Place of Movement in Consciousness,' *Psychol. Rev.*, 1911, 83–99.

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a condition of attention, might be alleged as the factor which determines individual differences in the present work; and it might be argued, further, that this capacity for motor adjustment was specific, that is, that it determines the ranking of attentions in the cases here in question, where attention is given for the purpose of reacting, but might not do so in other cases. In the present work, which measures attention as it occurs in conjunction with the purpose of reacting as quickly as possible, the factor of preparedness for action is conspicuously present. Indeed, to say that the stimulus meets with a high degree of preparedness to react is here taken to mean that it meets with a high degree of attention. But in what does the preparation to react consist? Does it consist in preparation of the *motor* centers or in a general cortical adjustment? That the motor centers are not actually innervated during the preparatory interval is evident from the fact that, with practiced subjects, the hand rests quietly on the reaction key, without even an increase in muscular tonus. The preparation for a given act does not consist, then, in innervation of the motor centers which control that act. To postulate any sort of 'openness' or preparedness other than actual innervation is to engage in mere speculation, to raise many problems and to solve none. That the preparatory state involves contraction of other muscles than those to be used in the reaction movement, such as those involved in breathing and in the adjustment of the sense organ for the proper reception of the stimulus, is known; but these adjustments are involved in any act of attention about as much as in the cases used in the present work. Moreover, they vary in each of the four cases used. They are not the same in a reaction to sound as in a reaction to light. It is evident, consequently, that the individual differences in attention found in the present investigation can not be due to some specific capacity for keeping 'open' certain motor centers.

I have elsewhere discussed at length what we must suppose to be the nature of the preparation to react if we are to explain the leading facts of reaction.[1] The conclusion reached, from

[1] 'Reactions to the Cessation of Stimuli and Their Nervous Mechanism,' *Psychol. Rev.*, 1915, 445–452.

the study of both beginning and cessation reactions, was that the preparation to react must consist in a widespread system of adjustments affecting the cerebral cortex as a whole. As a secondary effect of this adjustment, certain adjustments of the sense organs, the muscles concerned in breathing and other muscles may occur, but not any actual innervation of the motor response for which preparation is made. Preparation for an act, then, does not mean, primarily, a preparation of the motor area which controls that act. It is a disposition of the whole cortical system, that is, it is 'pre-motor.' One hardly needs to raise the question whether the adjustment of the nervous system in preparation for a reaction is a condition or a result of attention. The adjustment is the physiological parallel of attention.

From this point of view, we may state the problem of a faculty of attention in physiological terms. The act of attention involves a widespread cortical adjustment. Is there any general factor that comes in as a constant which helps to determine the efficiency of the adjustment in spite of variation in its type? That the conditions of the present research are satisfactory for a study of this problem is evident, for each of the four cases used clearly involves a widely different type of adjustment. The adjustment varies in pattern with the mode of stimulus to which the reaction is to occur, and is different in a simple reaction from what it is in a choice reaction. If, in spite of this variation in type of cortical adjustment, we find the same individual differences in its efficiency in all the four cases used, it must be concluded that these differences are general, individual differences.

Any individual differences in such specific factors as muscular strength or the nature of the reaction movement would not at all affect such measurements as the present ones, since they consist, not in reaction times, but in differences between two sets of reaction times of the same individual. Such individual peculiarities would, therefore, constitute a constant which would not affect a difference in reaction times any more than would a constant error in the chronoscope.

Without further discussion, it may be said that all the *c*

factors were the same for all subjects, unless the subjects differed in specific attention capacity in the four cases of attention used. The primary object of the present investigation is just to determine whether individuals do differ in specific attention capacities, that is, whether an individual may have good attention in the case of one type of mental process, and a relatively inferior one in another case, all conditions other than the type of mental process remaining constant. Of course, if other conditions of attention than the type of mental process were changed, and changed unequally for different individuals, in passing from one case to another, we would not expect a given individual to maintain the same standing in different cases. But it might be supposed, for example, that an individual would have good attention for a light stimulus and poor attention for a touch stimulus, even though no other differences in the conditions of attention existed in the two cases than that one was attention to light and the other attention to touch.

Since three of the four cases of attention used in the present investigation were produced by the use of three different modes of stimulus, the investigation may in large part be regarded as a study of mode of stimulus as a condition of attention. The case of attention with choice reactions was not studied so intensively, and only with ten of the twelve subjects. Consequently, the results with the three modes of stimuli will be presented first, and those with choice reactions afterwards, as supplementary data.

It has long been established that reaction time varies with the mode of stimulus, that at moderately strong intensities of stimulus the reaction time for sound is slightly longer than for touch and shorter than for light. Various explanations have been offered. The commonest view has been that the variation in reaction time with mode of stimulus is due to variation in the latent period of sensory stimulation. I have recently pointed out, however, that this view is untenable. Reaction time to the cessation of a stimulus equals that to its beginning, no matter what the mode of stimulus; and the latent period of stimulation cannot account for the fact that

both the beginning and the cessation reactions are shorter in the case of touch and sound than in the case of light. For this reason, as well as others, I have urged that the only acceptable explanation of the difference in reaction time with difference in mode of stimulus is to be found in the rôle played by the processes of the central nervous system, especially those which may be presumed to form the physiological aspect of attention. In confirmation of this position, I referred to unpublished results which showed that the degree of attention attracted by a light stimulus was less than that attracted by either a sound or touch stimulus.[1] These results are herewith presented.

The method used for measuring attention may be designated a *detraction* method, since it takes as the measure of degree of attention the effect produced by a detractor. A detractor is any condition or circumstance unfavorable to a high degree of attention. The detraction effect here used as the measure of degree of attention is that produced in reaction time by a set of unfavorable preparatory intervals, a set of irregularly mixed intervals varying in length from 4 to 20 seconds. The average reaction time with such a set of intervals is invariably greater than with a regularly repeated preparatory interval of 2 seconds; and the difference in reaction time obtained with the two sorts of preparatory intervals is a measure of the detraction effect. That this detraction effect is due solely to the fact that the unfavorable intervals allow a lesser degree of adaptation of attention to the reaction, is not only highly plausible from the absence of other explanation, but is a proposition which I have been able abundantly to confirm by showing that whenever the degree of attention was varied by varying any of its determinants, the magnitude of the detraction effect likewise varied.

That the detraction effect increases whenever the degree of attention decreases, has been shown by variation in a number of conditions of attention.[2] Thus, a decrease in the

[1] 'Reactions to the Cessation of Stimuli and Their Nervous Mechanism,' *Psychol. Rev.*, 1915, 444.

[2] Woodrow, 'The Measurement of Attention,' *Psychol. Monog.*, 1914, Vol. XVII., 5, pp. 158.

intensity of the stimulus, the most important of the objective conditions of attention, causes a marked increase in the prolongation in reaction time produced by the unfavorable intervals. Similarly, a decrease in the size of the change in intensity, in the case of reactions to a change, causes an increase in the detraction effect. An increase is likewise produced by ordinary distractors, such as unpleasant electrical shocks given the subject at the same time that he is reacting; by requiring the subject to carry on simultaneously some other task; or by having him imbibe of alcoholic liquor. The detraction effect is very much greater in children than in adults, a point on which additional data is presented below. In view of these results, there seems little room for doubt that degree of attention is measured by the reciprocal of the absolute detraction effect. Moreover, in a special study of 'Outline as a Condition of Attention,'[2] I have shown that the conclusions arrived at by the use of the detraction method completely correspond with those reached by means of introspections of clearness by practiced subjects.

The conditions of attention, to the study of which I have so far applied this method, it will be observed, are among those which were already best established. Such conditions were taken up first, as my interest was mainly in the method. The results, however, have been so uniformly corroborative of the validity of the method employed, that it seems safe to undertake now the study of conditions which it has hitherto been impossible to study for the lack of a suitable method. The effect of mode of stimulus has been taken up first, because of its bearing upon the question of a single faculty of attention, one of the most fundamental psychological questions now pressing for solution.

The intensities of stimuli used were probably about what are ordinarily regarded as moderate. They were not measured, but were kept as constant as possible throughout the work. It is known that, as long as the intensity of stimulus remains moderately strong, slight variations in intensity produce no reliable difference in reaction time; and when there

[1] This JOURNAL, Vol. I, 1916, 23–40.

is no difference in reaction time, all conditions remaining the same, there is in all probability no difference in the degree of attention. Any variations that could have occurred in the intensity of either the sound or light stimuli must have been very slight indeed, as such were never detected. The sound stimulus consisted in the click of an electric sound hammer (Wundt's), and the light stimulus in the flash of a Geissler's tube. As the electric current was supplied in both cases from a storage battery of 12 volts, there was no difficulty in maintaining a constant intensity. In the case of the touch stimulus, however, some difficulty was experienced. This stimulus consisted in an electric current, interrupted 250 times per second, led off from the secondary coil of an inductorium, to two large test-tubes filled with salt water, in each of which the subject placed a finger of his left hand. The interruptions of the current were made by means of an electric tuning-fork with platinum contact, and the intensity of the current varied considerably with the adjustment of the fork. While necessary readjustments of the fork were made with the greatest care, and at each sitting the subjects were asked whether the current seemed the same as usual, there is no doubt, especially when variations in the bodily resistance are considered, that only an approximate constancy was maintained.

As an index of the intensities of the stimuli used, the average reaction time with the regularly repeated 2 seconds interval, for the four quickest adults, may be cited. The average for touch was 125 σ; for sound, 144 σ; and for light, 175 σ. These times are perhaps rather long for practiced subjects, but this is readily accounted for by the fact that the subjects had to react first to one kind of stimulus and then to another, and first with regular preparatory intervals and then with irregular. This variation in the type of their task no doubt tended to obliterate the increase in speed that would normally result from doing the same thing day after day.

With each mode of stimulus, 25 reactions with a regularly repeated preparatory interval of 2 seconds were taken first and then 25 with irregular preparatory intervals. The 50

reactions required about 20 minutes. Then the experiment was repeated with a different stimulus. At one sitting of an hour, 150 reactions were taken, making three separate measurements of attention. Not more than two of these measurements were the measurements at present under discussion, that is, measurements of degree of attention in the case of reactions to moderately strong touch, sound and light stimuli. This is due to the fact that work was carried on at the same time on the degree of attention with choice reactions and with a weak light stimulus. The results with choice reactions will be presented later. Those with the weak light merely confirmed others already published, and will not be presented. The order in which the different measurements were made varied each day, the object being to take each kind of reaction about equally often at the beginning, middle, and end of the hour and to have each kind of reaction preceded in turn by each of the others. This constant change in each day's program undoubtedly accounts for a good deal of the variability of the measurements made on different days, and makes it impossible to reckon with any reliability to what extent the variation from day to day in the measurements obtained represents a true variation in degree of attention. Measurements with different modes were taken either on different days or at different places on the same day's program, and consequently it is not surprising that no reliable correlation was found to exist in their variation. The measurements with each mode may be safely regarded as independent series of measurements.

Twelve subjects were used. Seven of these were adults; four, subjects *Ws*, *Hl*, *Ss* and *Sy*, were boys of twelve to fifteen years of age, and one, subject *Je*, was a boy of eight years. Of the adults, four had had previous work in reaction time. These were subjects *St*, *Ww*, *Sl*, and *Sm*. An endeavor was made to obtain nine or ten measurements of degree of attention with each subject with each mode of stimulus. With two subjects, however, a smaller number of measurements had to suffice, while with two other subjects twelve measurements were made.

The reaction times were measured by a Hipp's chronoscope, controlled by a Wundt's fall-hammer. It seems unnecessary to describe the apparatus in detail, as it was in general like that already described in other work, and followed in the main the conventional reaction time arrangement. The warning signal was in all cases a click of a small sound hammer. The interval between warning signal and stimulus was judged by means of a head-receiver connected with an electric metronome beating seconds. The 2 seconds intervals were given as accurately as possible, and the whole series of reactions run off regularly and quickly. The experimental work occupied the whole of one summer, the total number of reactions here reported being 19,350. An assistant acted as experimenter in about one third of the work and also computed most of the averages and mean variations.[1]

The results with the different modes of stimuli are presented in Tables I. and II. Table I. gives the results of each measurement with each subject. As already explained, all three modes were never used at the same sitting, so that the three measurements given on the same line of the table were not made during the same hour. The measurements with each mode, however, are listed in the order in which they were obtained. The average reaction times with the 2 seconds intervals and with the irregular intervals are given in σ, and the absolute mean variation of each average given immediately below it in italics. The difference in the reaction time obtained with the two kinds of preparatory intervals is given in the columns headed $1/A$. This heading signifies that the greater this difference the less the degree of attention, A standing for degree of attention. Table II. is a summary of Table I.

Tables I. and II. establish certain conclusions beyond question. For every individual, the prolongation in reaction time produced by the irregular intervals, that is, the magnitude $1/A$, is greater with light than with either sound or touch. Since this prolongation varies inversely with the

[1] Assistance in this work was secured through a grant from the Research Fund of the University of Minnesota.

TABLE I

MEASUREMENTS OF ATTENTION WITH DIFFERENT MODES OF STIMULI

N, for each average, = 25. Total No. of reactions = 17,250.

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*	2 Secs.	Irreg.	1/*A*
St	119	169	50	124	163	39	166	219	53
	7	*10*		*11*	*26*		*15*	*18*	
"	114	163	49	150	198	48	164	210	46
	7	*12*		*10*	*28*		*27*	*18*	
"	111	160	49	154	206	52	162	209	47
	8	*19*		*17*	*30*		*18*	*20*	
"	123	168	45	142	174	32	174	216	42
	10	*13*		*11*	*16*		*17*	*15*	
"	125	174	49	155	202	47	164	219	55
	8	*17*		*13*	*29*		*15*	*17*	
"	117	159	42	148	210	62	183	226	43
	6	*8*		*13*	*30*		*22*	*22*	
"	128	175	47	157	216	59	172	221	49
	9	*14*		*9*	*25*		*21*	*13*	
"	138	170	32	152	200	48	189	248	59
	9	*8*		*17*	*20*		*14*	*21*	
"	133	171	38	171	202	31	177	229	52
	10	*8*		*13*	*17*		*18*	*18*	
"	146	176	30	180	237	57	202	244	42
	9	*11*		*13*	*14*		*23*	*14*	
"	149	180	31	165	217	52	191	244	53
	10	*12*		*12*	*12*		*13*	*13*	
"	138	183	45	170	215	45	182	247	65
	11	*15*		*15*	*16*		*18*	*18*	
Av.	**128**	**170**	**42**	**155**	**203**	**48**	**177**	**228**	**51**
Av. M. V.	*9*	*12*		*13*	*22*		*18*	*17*	
Ww	120	182	62	138	197	59	162	228	66
	13	*18*		*19*	*25*		*18*	*21*	
"	112	178	66	131	208	77	172	237	65
	13	*21*		*11*	*25*		*14*	*18*	
"	115	154	39	120	197	77	185	254	69
	16	*24*		*21*	*17*		*13*	*36*	
"	131	174	43	140	188	48	169	231	62
	11	*14*		*20*	*26*		*16*	*24*	
"	115	151	36	142	177	35	166	246	80
	10	*16*		*14*	*15*		*15*	*22*	
"	118	169	51	132	167	35	163	235	72
	17	*13*		*17*	*15*		*13*	*18*	
"	119	155	36	136	185	49	162	231	69
	9	*13*		*16*	*10*		*15*	*18*	
"	105	142	37	141	176	35	163	241	78
	7	*12*		*8*	*24*		*10*	*19*	
"	111	149	38	130	199	69	166	248	82
	12	*15*		*10*	*20*		*10*	*21*	
"	110	151	41	128	194	66	165	228	63
	11	*14*		*12*	*18*		*12*	*19*	
Av.	**116**	**161**	**45**	**134**	**189**	**55**	**167**	**238**	**71**
Av. M. V.	*12*	*17*		*15*	*20*		*14*	*22*	

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A
Ll	123	168	45	149	192	43	177	212	35
	24	*19*		*23*	*15*		*9*	*34*	
"	128	178	50	140	176	36	166	215	49
	15	*17*		*8*	*26*		*17*	*14*	
"	135	171	36	144	186	42	172	222	50
	6	*10*		*10*	*39*		*10*	*23*	
"	125	165	40	137	181	44	169	221	52
	21	*22*		*13*	*24*		*10*	*20*	
"	127	177	50	141	185	44	178	226	48
	12	*25*		*16*	*25*		*16*	*10*	
"	128	183	55	146	180	34	163	231	68
	22	*23*		*7*	*26*		*15*	*22*	
"	120	182	62	152	195	43	172	225	54
	23	*16*		*25*	*16*		*12*	*23*	
"	140	167	27	136	170	34	159	217	58
	13	*14*		*12*	*10*		*9*	*26*	
"	113	161	48	130	172	42	171	225	54
	9	*13*		*11*	*25*		*9*	*14*	
"	121	158	37	138	175	37	172	227	55
	10	*12*		*12*	*17*		*12*	*15*	
Av.	**126**	**171**	**45**	**141**	**181**	**40**	**170**	**222**	**52**
Av. M. V.	*16*	*17*		*14*	*22*		*12*	*21*	
Ht	145	215	70	146	204	58	194	258	64
	23	*26*		*18*	*18*		*23*	*38*	
"	165	227	62	181	254	73	212	281	69
	19	*22*		*26*	*24*		*38*	*25*	
"	180	241	61	194	254	60	220	278	58
	23	*21*		*11*	*34*		*17*	*17*	
"	165	209	44	211	273	62	197	244	47
	13	*27*		*19*	*19*		*22*	*26*	
"	151	202	51	169	219	50	207	280	73
	23	*23*		*22*	*33*		*20*	*38*	
"	167	226	59	154	214	60	182	246	64
	13	*21*		*14*	*16*		*26*	*35*	
"	132	190	58	155	199	44	207	266	59
	12	*15*		*16*	*15*		*16*	*25*	
"	165	199	34	185	221	36	187	237	50
	18	*14*		*16*	*17*		*18*	*10*	
"	139	181	42	150	198	48	206	273	67
	8	*11*		*12*	*18*		*11*	*25*	
Av.	**157**	**210**	**53**	**171**	**226**	**55**	**201**	**262**	**61**
Av. M. V.	*16*	*20*		*17*	*22*		*21*	*26*	
Bs	139	216	77	154	211	57	196	287	91
	24	*28*		*18*	*28*		*15*	*25*	
"	135	208	73	138	211	73	210	278	68
	12	*20*		*25*	*28*		*13*	*25*	
"	126	184	58	146	211	65	188	258	70
	8	*10*		*13*	*26*		*10*	*18*	
"	127	182	55	125	180	55	182	256	74
	10	*12*		*14*	*17*		*12*	*20*	
"	129	187	58	160	215	55	182	260	88
	10	*18*		*15*	*26*		*17*	*20*	
"	131	185	54	148	218	70	171	249	78
	12	*17*		*9*	*18*		*12*	*22*	
"	122	194	72	139	222	83	168	262	94
	12	*21*		*16*	*23*		*12*	*21*	
Av.	**130**	**194**	**64**	**144**	**209**	**65**	**185**	**264**	**79**
Av. M. V.	*13*	*18*		*16*	*24*		*13*	*23*	

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A
Bt	151	227	76	149	228	79	212	279	67
	20	*31*		*18*	*48*		*37*	*33*	
"	169	219	50	189	253	64	209	308	99
	21	*21*		*22*	*38*		*32*	*35*	
"	169	233	64	176	233	57	221	286	65
	12	*26*		*25*	*23*		*24*	*26*	
"	194	241	47	166	217	51	209	287	78
	33	*24*		*10*	*22*		*22*	*35*	
"	167	224	57	162	243	81	190	278	88
	10	*31*		*14*	*29*		*25*	*29*	
"	178	233	55	166	211	45	195	294	99
	18	*26*		*13*	*18*		*15*	*34*	
"	149	235	86	151	217	66	212	288	76
	15	*25*		*12*	*26*		*30*	*30*	
"	154	207	53	153	234	81	205	261	56
	15	*18*		*11*	*21*		*11*	*15*	
"	151	207	54	163	236	73	216	274	58
	20	*14*		*10*	*16*		*15*	*26*	
"	149	197	48	159	226	67	208	296	88
	9	*13*		*16*	*20*		*14*	*23*	
Av.	**163**	**222**	**59**	**165**	**231**	**66**	**208**	**285**	**77**
Av. M. V.	*17*	*26*		*15*	*26*		*23*	*29*	
Lm	144	214	70	148	237	89	209	304	95
	12	*31*		*13*	*19*		*25*	*26*	
"	146	233	87	170	248	78	230	328	98
	13	*30*		*14*	*24*		*23*	*42*	
"	166	239	73	173	282	109	224	321	97
	16	*17*		*28*	*45*		*22*	*32*	
"	151	247	96	186	281	95	240	330	90
	25	*34*		*29*	*36*		*14*	*26*	
"	144	222	78	168	260	92	230	323	93
	21	*26*		*21*	*29*		*19*	*28*	
"	172	259	87	198	290	92	209	301	92
	27	*53*		*23*	*30*		*14*	*27*	
"	157	254	97	184	279	95	240	328	88
	15	*28*		*25*	*49*		*26*	*42*	
"	153	247	94	185	282	97	210	310	100
	28	*18*		*21*	*44*		*16*	*26*	
"	145	230	85	178	269	91	217	323	106
	11	*20*		*31*	*41*		*17*	*30*	
"	152	242	90	172	278	106	178	279	101
	14	*27*		*21*	*27*		*32*	*24*	
"	151	255	104	172	272	100	210	314	104
	26	*23*		*18*	*28*		*21*	*23*	
"	127	210	83	174	260	86	226	327	101
	12	*17*		*26*	*24*		*35*	*29*	
Av.	**150**	**237**	**87**	**175**	**269**	**94**	**219**	**316**	**97**
Av. M. V.	*18*	*27*		*23*	*33*		*22*	*30*	

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A
Ws	161	248	87	180	278	98	223	341	118
	27	*52*		*12*	*46*		*40*	*73*	
"	154	251	97	178	228	50	224	306	82
	22	*41*		*25*	*25*		*22*	*33*	
"	170	230	60	156	238	82	197	303	106
	26	*30*		*18*	*33*		*15*	*44*	
"	151	235	84	145	204	59	214	304	90
	14	*43*		*19*	*29*		*26*	*41*	
"	146	227	81	148	221	73	195	278	83
	29	*44*		*15*	*45*		*27*	*28*	
"	122	205	83	151	232	81	219	302	83
	19	*26*		*18*	*45*		*25*	*35*	
"	134	208	74	142	223	81	219	304	85
	15	*31*		*16*	*35*		*32*	*40*	
"	130	231	101	138	240	102	198	306	108
	9	*34*		*17*	*43*		*16*	*44*	
"	133	244	111	137	246	109	202	318	116
	13	*48*		*14*	*35*		*18*	*29*	
"	130	206	76	134	218	84	213	306	93
	15	*26*		*19*	*27*		*31*	*39*	
Av.	**143**	**228**	**85**	**151**	**233**	**82**	**210**	**306**	**96**
Av. M. V.	*19*	*38*		*17*	*36*		*25*	*41*	
Ls	163	277	114	162	249	87	198	308	110
	25	*37*		*26*	*30*		*23*	*44*	
"	151	253	102	158	264	106	244	365	121
	14	*36*		*27*	*41*		*48*	*42*	
"	172	277	105	159	280	121	195	329	134
	21	*56*		*16*	*42*		*25*	*40*	
"	184	259	75	185	289	104	239	390	151
	27	*26*		*25*	*53*		*23*	*55*	
"	166	254	88	180	276	96	224	337	113
	10	*27*		*25*	*46*		*34*	*44*	
"	172	252	80	172	255	83	227	354	127
	15	*24*		*24*	*25*		*48*	*42*	
"	176	244	68	192	319	127	249	375	126
	22	*24*		*30*	*36*		*35*	*52*	
"	192	250	58	184	259	75	231	359	128
	24	*34*		*28*	*32*		*20*	*23*	
"	180	293	113	175	304	129	200	309	109
	12	*48*		*23*	*50*		*20*	*26*	
"	157	251	94	153	267	114	214	322	108
	12	*33*		*15*	*31*		*22*	*34*	
Av.	**171**	**261**	**90**	**172**	**276**	**104**	**222**	**345**	**123**
Av. M. V.	*18*	*35*		*24*	*39*		*30*	*40*	
Je	253	392	139	268	461	193	285	462	177
	34	*43*		*39*	*74*		*39*	*115*	
"	229	397	168	259	423	164	256	451	195
	24	*68*		*18*	*37*		*44*	*62*	
"	200	372	172	203	361	158	242	459	217
	26	*54*		*34*	*53*		*45*	*63*	
"	198	312	114	225	402	177	269	440	171
	15	*58*		*28*	*41*		*48*	*47*	
"	196	338	142	214	395	181	289	435	146
	19	*47*		*29*	*53*		*45*	*77*	
Av.	**215**	**362**	**147**	**233**	**408**	**175**	**268**	**449**	**181**
Av. M. V.	*23*	*54*		*29*	*51*		*44*	*73*	

Subj.	Touch			Sound			Light		
	2 Secs.	Irreg	1/A	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A
Hl.....	164	275	111	157	274	117	211	331	120
	14	*42*		*19*	*54*		*25*	*44*	
"	156	232	76	164	282	118	210	316	106
	10	*26*		*21*	*59*		*12*	*45*	
"	154	260	106	174	254	80	199	294	95
	11	*38*		*27*	*48*		*27*	*38*	
"	173	245	72	161	228	67	214	321	107
	21	*31*		*27*	*30*		*14*	*46*	
"	160	287	127	177	326	149	234	350	116
	14	*47*		*27*	*90*		*26*	*48*	
"	195	300	105	161	291	130	229	361	132
	22	*50*		*26*	*70*		*24*	*52*	
"	156	264	108	167	252	85	226	388	162
	13	*30*		*16*	*69*		*35*	*52*	
"	166	249	83	188	324	136	233	399	166
	15	*35*		*19*	*33*		*18*	*41*	
"	187	264	77	163	319	156	215	387	172
	17	*31*		*20*	*38*		*22*	*44*	
Av.....	**168**	**265**	**97**	**168**	**283**	**115**	**219**	**350**	**131**
Av. M. V.....	*15*	*36*		*22*	*55*		*23*	*46*	
Sy.....	220	336	116	186	327	141	255	386	131
	29	*46*		*17*	*43*		*27*	*60*	
"	192	313	121	208	334	126	228	385	157
	24	*50*		*16*	*49*		*23*	*85*	
"	212	340	128	233	432	199	258	409	151
	56	*60*		*31*	*53*		*38*	*64*	
"	156	258	102	191	330	139	131	387	156
	13	*34*		*23*	*46*		*28*	*53*	
"	158	316	158	187	335	148	222	370	148
	18	*38*		*18*	*22*		*26*	*48*	
"	158	269	111	182	325	143	232	351	119
	17	*43*		*20*	*37*		*29*	*36*	
"	161	281	120	192	309	117	216	381	165
	15	*40*		*18*	*38*		*17*	*42*	
"	191	343	152	185	332	147	215	368	153
	25	*23*		*22*	*53*		*22*	*39*	
"	158	255	97	191	331	140	240	383	143
	15	*38*		*13*	*41*		*33*	*37*	
"	165	298	133	189	323	134	225	373	148
	19	*39*		*19*	*43*		*21*	*40*	
Av.....	**177**	**301**	**124**	**194**	**337**	**143**	**232**	**379**	**147**
Av. M. V.....	*23*	*41*		*20*	*43*		*26*	*50*	

TABLE II

A SUMMARY OF TABLE I

Subj.		Touch			Sound			Light		
		2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A	2 Secs.	Irreg.	1/A
St.	Av.	128	170	42	155	203	48	177	228	51
	" M. V.	*9*	*12*		*13*	*22*		*18*	*17*	
Ll.	"	126	171	45	141	181	40	170	222	52
	" "	*16*	*17*		*14*	*22*		*12*	*21*	
Ww.	"	116	161	45	134	189	55	167	238	71
	" "	*12*	*17*		*15*	*20*		*14*	*22*	
Ht.	"	157	210	53	171	226	55	201	262	61
	" "	*16*	*20*		*17*	*22*		*21*	*26*	
Bs.	"	130	194	64	144	209	65	185	264	79
	" "	*13*	*18*		*16*	*24*		*13*	*23*	
Bt.	"	163	222	59	165	231	66	208	285	77
	" "	*17*	*26*		*15*	*26*		*23*	*29*	
Ws.	"	143	228	85	151	233	82	210	306	96
	" "	*19*	*38*		*17*	*36*		*25*	*41*	
Lm.	"	150	237	87	175	269	94	219	316	97
	" "	*18*	*27*		*23*	*33*		*22*	*30*	
Ls.	"	171	261	90	172	276	104	222	345	123
	" "	*18*	*35*		*24*	*39*		*30*	*40*	
Hl.	"	168	265	97	168	283	115	219	350	131
	" "	*15*	*36*		*22*	*55*		*23*	*46*	
Sy.	"	177	301	124	194	337	143	232	379	147
	" "	*23*	*41*		*20*	*43*		*26*	*50*	
Je.	"	215	362	147	233	408	175	268	449	181
	" "	*23*	*54*		*29*	*51*		*44*	*73*	

degree of attention, it follows that the degree of attention of all the subjects was less in the case of reactions to light than in the case of reactions to sound or touch. We may conclude, consequently, that just as we say that the reaction time to light is ordinarily longer than to sound or touch, so may we say that the degree of attention given to a light stimulus is ordinarily less than that given to a sound or a touch stimulus.

As regards the difference in degree of attention between touch and sound, the data are somewhat less uniform, but clearly show a general tendency for the degree of attention to touch to exceed that to sound. The average per cent. by which $1/A$ with sound exceeds $1/A$ with touch is almost as great as that by which $1/A$ with light exceeds $1/A$ with sound. There are, however, two individuals, subjects *Ll*, and *Ws*, with which degree of attention, as measured, is greater with sound than with touch. Two exceptions out of twelve is not sufficient to invalidate the conclusion that, as a

general rule, attention to touch is better than to sound. By saying this is a general rule, it is meant that, if all errors of measurement and ignored causes of variability were ruled out, there would be no exceptions. The two exceptions may be regarded as natural in view of the variability of the measurements.[1] Two undoubted sources of this variability, which were not eliminated, were the variations, already referred to, in the intensity of the electric current used as the touch stimulus, and variations in the sensitivity of the sense organs. None of the subjects had any marked abnormalities of sensory acuity, but that differences great enough to affect the outcome of the measurements may have existed is rather probable. Subject *Ll*, one of the two who showed a better attention to sound than to touch, was known to have exceptionally acute auditory sensitivity, both from tests with the Lehmann acoumeter and from general observation, while, on the other hand, she frequently complained of the weakness of the electric current, and makes the statement that her fingers often become rather numb because, according to her physician, of poor circulation. For the other exceptional subject, *Ws*, who showed a value of $1/A$ of 83 with sound and 85 with touch, no explanatory data were obtained.

The preceding data show that the result obtained from the measurement of the degree of an individual's attention by the method here employed depends upon the mode of stimulus. Measurements made with different modes of stimuli are made, therefore, as it were, upon different scales. The question now arises, whether, after determining the relation between the three scales, and allowing for this difference in scale, a person's attention would be found to be equally good in all three cases. Of course, the allowance must be made in the same way for all individuals and must be based on the ascertained relationship between the three scales. If, after making such allowance, we find the degree of attention for each individual to be the same, we have definite proof of a common factor of attention, that is, of general conditions which remain the same for a given individual in spite of the variation in mode.

[1] See Tables III. and IV.

Instead of attacking the problem by the method here proposed, that is, by making an allowance for the effect of mode, many psychologists would no doubt be inclined to use the method of correlation. The problem of a faculty of attention may be clearly stated in terms of correlation. If the correlation of degree of attention as involved in one type of mental activity, *e. g.*, in a touch reaction, with degree of attention in a different type, *e. g.*, in a sound reaction, were found to be perfect, we would have quite conclusive evidence that the degree of attention of any individual was determined in the two cases by the same factors. Stated in terms of rank, it is the problem whether each individual maintains the same rank in different cases of attention in spite of variation in the specific psycho-physical constitution of the different cases, *e. g.*, in spite of variation in mode of stimulus.

The present investigation, however, was not planned with the idea of treating the data by the method of correlation. Main reliance was placed on the method already mentioned, which is explained in detail below. It was thought preferable to use a relatively small number of subjects and make as accurate measurements as possible with each subject, repeating the measurements a sufficient number of times to obtain fairly reliable averages. Five youthful subjects were used in addition to the adults in order to secure a greater variation in degree of attention than would be found in even a very large group composed exclusively of normal adults. It did not seem particularly important to what the differences in attention were due, whether to growth or other factors. The important question was whether the individual differences in degree of attention would be found always the same. The research as planned, then, is somewhat unsuited to the application of correlational methods, both because of the limited number of cases and the fact that different ages were intentionally included. For those who are accustomed to correlational methods, however, it may be interesting to note the coefficients of correlation which may be obtained between the values of $1/A$ for sound, touch and light. The Pearson coefficients

$$\left(r = \frac{\Sigma(xy)}{n\sigma_x\sigma_y}\right)$$

are in all cases practically 1.00, and remain so when calculated for age constant, by the formula,[1]

$$r_{12.3} = \frac{r_{12} - r_{13} \cdot r_{23}}{\sqrt{(1 - r_{13}^2)\ (1 - r_{23}^2)}}.$$

The correlations are as follows:

r (sound and light) = 1.00; age constant = 1.00;
r (sound and touch) = .99; " " = .99;
r (touch and light) = .98; " " = .96.

The correlation, in the group used, between age and the reciprocal of degree of attention ($1/A$), in the case of touch was $-$ .78; of sound, $-$ .78; and of light, $-$ .76. The fact that the correction for difference in age leaves the correlations of the values of $1/A$ for the three different modes practically unchanged, shows that the high correlations obtained are not due to difference in age. As regards the number of individuals, it is certain that the correlation obtained would remain approximately the same no matter what the number of subjects used, as it will be shown below that the degree of attention of any individual, no matter what his age, is the same, within the limits of the inaccuracy of measurement, in the case of all three modes, when reduced to the same scale.

The method used for equating the measurements made with the different modes is simple. It may be seen from Table III. that the average value of $1/A$ for the entire group of individuals is 78.2 for touch, 86.8 for sound, and 97.2 for light, that is, that the value of $1/A$ for the group is 11 per cent. greater for sound than for touch and 12 per cent. greater for light than for sound. These percentages for the group very nearly coincide with the average of the percentages in the case of each individual; and, though they show a large variation among the different individuals, there is no general tendency for them either to increase or decrease with increase in the value of $1/A$. It will, then, be a very close approxi-

[1] Yule, 'An Introduction to the Theory of Statistics,' 1911, 247.

TABLE III

THE OBTAINED AVERAGE VALUES OF THE RECIPROCAL OF DEGREE OF ATTENTION WITH MEASURES OF THEIR UNRELIABILITY

$1/A$ = average value of reciprocal of degree of attention.
N = No. of measurements.
S.D. = standard deviation of the distribution.
σ_m = standard error of the mean.

Subj.	Touch			Sound			Light			N
	$1/A$	S.D.	σ_m	$1/A$	S.D.	σ_m	$1/A$	S.D.	σ_m	
St.....	42	7	2.1	48	9	2.7	51	7	2.0	12
Ll.....	45	10	3.1	40	4	1.2	52	8	2.5	10
Ww.....	45	12	3.7	55	16	5.1	71	7	2.2	10
Ht.....	53	11	3.6	55	10	3.5	61	8	2.7	9
Bs.....	64	9	3.4	65	10	3.7	79	9	3.5	7
Bt.....	59	12	4.0	66	15	4.7	77	15	4.8	10
Ws.....	85	14	4.4	82	17	5.5	96	14	4.3	10
Lm.....	87	9	3.0	94	8	2.6	97	5	1.7	12
Ls.....	90	18	5.6	104	18	5.6	123	13	4.1	10
Hl.....	97	18	6.1	115	29	9.9	131	27	9.1	9
Sy.....	124	19	5.9	143	21	6.5	147	13	4.0	10
Je.....	147	21	9.4	175	13	5.7	181	23	10.1	5
Av.....	78.2			86.8			97.2			

mation to the truth, to assume that $1/A$ tends in general, with the subjects used, to be 11 per cent. greater in the case of sound than in the case of touch and 12 per cent. greater in the case of light than in the case of sound.

The correction for mode has been made, therefore, on the basis of the following equations:

$$\frac{1/A \text{ (sound)}}{1/A \text{ (touch)}} = \frac{86.8}{78.2} = 1.11 \text{ and } \frac{1/A \text{ (sound)}}{1/A \text{ (light)}} = \frac{86.8}{97.2} = .89.$$

The results with sound have been kept as a standard and those with touch and light reduced to this standard by multiplying the final values of $1/A$ by 1.11 in the case of touch and by .89 in the case of light. Another way of putting this is to say that the results for each individual with any one mode have been divided by the average for all individuals, and thus reduced to the same scale; only in order to keep the results with sound as a standard, the results have then been multiplied by 86.8, the average result with sound. This procedure is similar to that which would be used in reducing to the

same basis measurements made by different types of thermometers, providing the latter had the same zero point.

The results are presented in Tables III. and IV. It will be observed in Table IV. that, after allowance for the effect of mode has been made, there is a striking degree of similarity in the absolute values of $1/A$ for a given individual with the three different modes. The results for the three modes are not identical. It would be nothing short of a miracle if

TABLE IV

THE DEVIATIONS OF THE OBTAINED AVERAGES FROM THE MOST PROBABLE TRUE AVERAGES, AFTER REDUCTION OF ALL THE OBTAINED VALUES TO A COMMON SCALE

The deviations are expressed as fractions of the most probable standard errors (Av. σ_m) of the most probable true values (Av. $1/A$), in the columns headed d/σ_m.

Subj.	Touch			Sound			Light			Av. $1/A$	Av. σ_m
	$1/A$	σ_m	d/σ_m	$1/A$	σ_m	d/σ_m	$1/A$	σ_m	d/σ_m		
St.....	47	2.3	0.00	48	2.7	+0.44	46	1.8	−0.44	47	2.3
Ll.....	50	3.4	+2.17	40	1.2	−2.17	46	2.2	+0.44	45	2.3
Ww...	50	4.1	−1.68	55	5.1	−0.27	63	2.0	+1.89	56	3.7
Ht....	59	4.0	+0.91	55	3.5	−0.30	54	2.4	−0.61	56	3.3
Bs....	71	3.8	+0.57	65	3.7	−1.14	71	3.1	+0.57	69	3.5
Bt.....	66	4.4	−0.23	66	4.7	−0.23	69	3.9	+0.47	67	4.3
Ws....	94	4.9	+1.49	82	5.5	−1.06	86	3.8	−0.21	87	4.7
Lm....	97	3.3	+1.60	94	2.6	+0.40	87	1.5	−2.40	93	2.5
Ls....	100	6.2	−0.96	104	5.6	−0.19	110	3.7	+0.96	105	5.2
Hl....	108	6.8	−0.60	115	9.9	+0.24	117	8.1	+0.48	113	8.3
Sy....	138	6.5	0.00	143	6.5	+0.91	131	3.6	−1.27	138	5.5
Je.....	163	10.4	−0.48	175	5.7	+0.95	162	9.0	−0.60	167	8.4

they were, in view of the variability of the measurements, which the original data, presented in Table I., show to be considerable. The question arises, therefore, whether the results obtained with the different modes of stimuli agree, after correcting for mode, as closely as could be expected, in view of the variability of the measurements compared, even if their true value were identical. To determine this, the standard error of each average (σ_m) has been calculated and a comparison made, in Table V., of the correspondence between the variation found in the averages of each individual with the variation that would be expected in view of the standard errors, provided the variation were due solely to

errors of sampling. Since a number of measurements of $1/A$ were made for each individual with each mode, the data permit the calculation of the standard error for each average value of $1/A$ given in Table III. By the standard error of the average[1] is meant the standard deviation of the distribution (S. D., Table III.) divided by the square root of the number of cases (N, Table III.).

TABLE V

A COMPARISON OF THE OBTAINED DISTRIBUTION OF THE VALUES OF $1/A$ WITH THE DISTRIBUTION TO BE EXPECTED IF THE VARIATION IN THESE VALUES WITH MODE OF STIMULUS (AFTER REDUCTION TO A COMMON SCALE) WERE DUE TO CHANCE

d/σ_m	Expected	Obtained
0.00 to ± 0.25	20%	22.2%
± 0.25 " ± 0.53	20	22.2
± 0.53 " ± 0.84	20	13.9
± 0.84 " ± 1.28	20	22.2
± 1.28 " ± 2.58	19	19.4
± 2.58 " ± ∞	1	0.0

In reducing the measurements made with different modes to a single scale (Table IV.), the standard errors of the averages have been treated like the averages, that is, each average and its standard error has been multiplied by 1.11 in the case of touch and by .89 in that of light. This treatment gives the same standard error as would multiplying each of the original measurements of $1/A$ for touch by 1.11 and for light by .89 and calculating the standard errors anew; so that no new assumption is made in correcting the standard errors of the averages in the same way as the averages. If we assume that, in the measurements with touch, sound and light, we are really measuring the same general power of attention under three different conditions, it is evident that the most probable true value of this power of attention for a given individual is the average of the values obtained in the three cases. This average is given in Table IV. in the column

[1] Called by Thorndike the mean square deviation of the probable divergence of the true from the obtained average ($\sigma_{t.\ av.\ -\ obt.\ av.}$), 'Mental and Social Measurements,' 2d ed., 1913, 189.

headed "Av. $1/A$." Similarly, the most probable standard error of an average obtained for this true value, from the same number of measurements as used in the case of each mode, is the average of the standard errors in the three cases. For example, in the case of subject *St*, the three averages obtained for $1/A$, after eliminating the effect of mode, are given in Table IV. as 47, 48 and 46. The most probable true value of $1/A$, then, is 47. The standard errors of these averages, each based on twelve measurements, are 2.3, 2.7 and 1.8, respectively. The most probable standard error, then, for an average of twelve measurements[1] is the average of 2.3, 2.7 and 1.8, or 2.3. This most probable standard error is given in Table IV. in the column headed 'Av. σ_m.'

Now if the standard error of a mean of 12 measurements is 2.3, this implies that, if we determined independently a number of means each based on 12 measurements, these would tend to vary in such a way that 20 per cent. of them would not deviate from the average of all by more than $.025 \times 2.3$, 40 per cent. by more than 0.53×2.3, 60 per cent. by more than 0.84×2.3, etc.[2] It is easy to determine, therefore, whether the three obtained values of $1/A$ deviate from the most probable true value more than should be expected if the deviations were due merely to sampling. To determine this, the deviation of each average is expressed as a fraction of the most probable standard error (Table IV., columns headed d/σ_m). Altogether, there are thirty-six cases. It is true that these thirty-six cases are obtained by combining three cases only from each of twelve subjects. The deviations for each subject, however, are reckoned with respect to his own average in terms of the corresponding standard error. It is quite permissible, consequently, to group all cases together, to see to what extent the distribution of the obtained means corresponds with the distribution to be expected solely on account of the unreliability of the means.

Table V. shows that the distribution of the individual

[1] For the formula for calculating the standard error for the total number of measurements, that is, in the illustration, 36, see Yule, *op. cit.*, 142.

[2] See Thorndike, *op. cit.*, 198, or W. F. Sheppard, *Biometrika*, 1903, Vol. II., 182.

means from the most probable true means corresponds as closely as could be expected with the most probable distribution of these means if their variation with mode of stimulus were due to chance. The three average values of $1/A$ obtained with each individual with different modes of stimuli do not vary from each other, or from their own average, more than they would because of errors of sampling. In the case of each individual, a true value of $1/A$ may be assumed, namely, the most probable true value, and in no case will any of the obtained values differ from this assumed true value more than would be expected from the standard errors of the obtained means. All the means obtained with any individual may, then, be regarded as measurements of the same magnitude. Since the magnitude in question varies as the reciprocal of the degree of attention, each individual may be said to show the same degree of attention in all three cases, after proper allowance has been made for the specific factor of sensory mode.

As supplementary proof of the conclusions drawn from Tables III.–V., it may be mentioned that in no case does the difference in the mean values of $1/A$ obtained with any two different modes of stimulus, after reduction to a common scale, exceed three times the standard error of the difference.[1] Further, in no case does the deviation of any mean from the most probable true mean exceed three times the most probable standard error.

The preceding tables have shown that the degree of attention of an individual is affected by the mode of stimulus to which he attends, but that when the effect of mode is allowed for, the individual differences in degree of attention remain the same. The data demonstrate, therefore, that an individual has a certain general ability to attend which remains the same, relative to other individuals' abilities, in spite of variation in the type of mental process concerned. It might be objected that the preceding data show merely that an individual's attention is conditioned by general factors which remain constant in spite of variation in the mental content,

[1] For formula, see Yule, *op. cit.*, 342.

but that these general factors would not remain constant with variation in the form of mental process. To test the validity of this objection, degree of attention was measured in the case of ten of the objects, as it occurred in a choice reaction—a form of mental activity quite different from the simple reaction, and one which, according to the results obtained, conditions a quite different degree of attention than does the form of activity involved in the simple reaction.

The stimulus for the choice reactions consisted in an electric lamp placed behind an aperture in a dark box, and arranged so that its brightness could be made to either increase or decrease as the chronoscope circuit was closed. The lamp in the dark box, a 40-watt Mazda, was placed in series with two other 40-watt Mazda lamps, the latter two lamps being connected in parallel. The circuits were so arranged that as the switch in the chronoscope circuit was closed, either a 150-watt lamp was thrown in parallel with the pair of 40-watt lamps, thereby decreasing the resistance in the stimulus circuit, or one of the two 40-watt lamps in parallel was cut out, thereby increasing the resistance. When the resistance was decreased, the lamp in the dark box suddenly increased in brightness, and when the resistance was increased, the lamp decreased in brightness. The increase and decrease were about equally large changes. With the Mazda lamps used, these changes occurred with great suddenness, though of course there must have been some slight lag in both the increase and decrease. Since, however, both the increase and decrease were used equally often with both the regularly repeated and the irregularly mixed intervals, any lag in the change in brightness would be no drawback to the arrangement as regards the purpose of the present work, which uses merely the *difference* in reaction time with the two sets of preparatory intervals.

The sequence of increases and decreases in brightness was a chance one, and different at each measurement. It was the same, however, for both the regular and irregular intervals of any one measurement. Two reaction keys were employed, one for the right hand and one for the left. At

the warning signal, the subject had to press down on both keys. Then, if the stimulus increased in brightness, he reacted by removing the fingers of the right hand while keeping those of the left pressed down, while if the stimulus decreased in brightness, he reacted by raising the fingers of the left hand while keeping those of the right upon the key.

TABLE VI

A COMPARISON OF DEGREE OF ATTENTION IN THE CASE OF CHOICE REACTIONS WITH THAT IN THE CASE OF SIMPLE REACTIONS

Subj.	Choice				Av. $1/A \times 1.6$ (Simple)	Av. $1/A$ (Simple)
	N	2 Secs.	Irreg.	$1/A$ (Choice)		
St.....	6	239	307	68	75	47
		56	*57*			
Ll.....	3	238	301	63	72	45
		48	*53*			
Ww.....	4	296	387	91	90	56
		45	*68*			
Ht.....	4	273	358	85	90	56
		43	*59*			
Bs.....	4	302	437	135	110	69
		47	*70*			
Bt.....	2	345	461	116	107	67
		48	*61*			
Ws.....	5	353	496	143	139	89
		66	*71*			
Lm.....	5	332	492	160	149	93
		63	*75*			
Ls.....	4	322	496	174	168	105
		48	*91*			
Sy.....	5	436	642	206	221	138
		68	*79*			

The results are given in Table VI., the headings of which have the same significance as in Table I. For comparison of the degree of attention in choice reactions with that in the other cases already reported, in which simple reactions were employed, the average value of $1/A$ for the three other cases is given for each individual in the column headed 'Av. $1/A$ (Simple).' This is the value which is given in Table IV. as 'Av. $1/A$.' The average value of $1/A$ for the group is 1.6 as large for choice reactions as for the average of the other cases. Consequently, to facilitate the comparison of the individual differences in the case of choice reactions with those in the case of simple reactions, the average value

in the latter cases has been multiplied throughout by 1.6, and the result given in the column headed 'Av. $1/A \times 1.6$ (Simple).' As the number of measurements with choice reactions made with each individual is small, in some cases as low as two, only the average values are given, as in Table II. Both the averages and the mean variations in italics below them are the mean values obtained for a series of 25 reactions such as were taken in each measurement. In column N is given the number of measurements. The total number of reactions represented is 2,100.

The significance of Table VI. is made clear by a comparison of the column '$1/A$ (Choice)' with the column 'Av. $1/A \times 1.6$ (Simple).' While the two columns fall far short of being identical, there is nevertheless a very marked correspondence in the values obtained for each individual. On account of the small number of measurements made with the choice reactions, it does not seem worth while to make such comparisons in the present instance as in the case of the results obtained with the three modes of stimulus and simple reactions. Without any such comparison, it is yet perfectly obvious that the individual differences which exist with simple reactions remain substantially the same with choice reactions. It must be concluded that the individual differences in degree of attention obtained in the present work are not dependent upon the specific form of the mental process involved. These individual differences remain substantially the same in spite of variation in either the form or content of the mental processes, and must therefore be regarded as in all probability due to general conditions, which affect any act of attention of a given individual.

The preceding data establish the fact that there are general, individual conditions of attention. This, of course, does not mean that there are not also very many more or less specific conditions, conditions which apply only in one or a few cases of attention. In fact, two of these conditions, mode of stimulus and form of mental activity, are brought out by the above data. The specific conditions used in the present work are controllable, laboratory conditions. The data

show that it has been possible to keep these specific conditions the same for all subjects. Under ordinary circumstances, this would not be possible. If situations were used in which a certain body of knowledge or certain special aptitudes were conditions of attention, it would be impossible to demonstrate the existence of the general, individual factor—at any rate, with the same definiteness and precision as in the present investigation. One person, for example, might be able to give much better attention to an orchestral symphony than another, but the latter individual might give much better attention to a debate on socialism. This is due to the fact, that, in these cases of attention, the specific conditions are not the same, and cannot be made the same, for all individuals. When there is variation, from one case of attention to another, in the degree to which different individuals are favored with respect to the specific conditions, there will naturally be found variation in the ranking of the individuals. On the other hand, the present investigation shows that the individual differences in degree of attention persist unchanged in spite of variation in the specific conditions, when the latter are rendered equal for all subjects. Thus, as the result of the control of conditions in the laboratory, it is possible to demonstrate the existence of general factors. While these are not so obvious in ordinary life, the variety of cases employed in the present investigation is sufficient to render it highly probable that these general conditions are really general, that is, that they are operative in any case of attention whatsoever, inside or outside of the laboratory, tending to make the attention of any individual uniformly better or worse than that of another, but ordinarily obscured by inequality for different individuals in the specific conditions of each case of attention.

The present investigation has resulted in the experimental demonstration—probably the first such—of a general capacity for attention on the part of any individual. At the same time, it has been emphasized that the general conditions are never operative alone, but always in conjunction with certain more or less specific conditions. There is a certain similarity in

this position to Spearman's well-known theory of intelligence, called by him the theory of two factors.

Spearman has advanced the view " . . . that a person's success in any intellectual performance may be regarded as the joint product of two factors.

"The one is 'specific ability' for the performance in question with all its particular features."[1] The second is 'general ability.'[2] He writes that " . . . while the range of the specific factor is exceedingly narrow, that of the general factor is universal; and between these two there appears to be no intermediate."[3]

One might attempt to identify the individual's general capacity for attention with the factor called 'general ability,' and to analyze all the other conditions of attention into complexes of very specific elements. In such case, the present study might be regarded as merely a confirmation of Spearman's theory. The identification of general ability, or the essential factor therein, with capacity for attention has often been attempted,[4] but with questionable success. Spearman, himself, declines to admit such identification. Without doubt, the question of the identification of the general capacity for attention with general ability should be regarded as an open one. The data for a sure decision in the matter are as yet unobtained.

Summary

The present research consists in measurements of the degree of attention of each of twelve subjects in each of the following four cases: simple reactions to touch; to sound; to light; and choice reactions to an increase or decrease in the intensity of light. The first three cases, each of which involves a different mode of stimulus, studied are especially

[1] *Journ. of Abnorm. Psychol.*, 1914, 4, 219.

[2] *Op. cit.*, 221.

[3] *Op. cit.*, 221.

[4] Ferrier, 'The Functions of the Brain,' 2d ed., 1886; Binet, 'Attention et adaptation,' *Anneé psychol.*, 1899, 6, 393 and 395; Sollier, 'Psychologie de l'idiot et de l'imbecile,' 1901, 36–37 and 60–74; Wundt, 'Physiologische Psychologie,' 6th ed., 1908, Vol. I., 378–386; Burt, 'Experimental Tests of General Intelligence,' *Brit. Journ. of Psychol.*, 1909, 3, 166; Abelson, 'The Measurement of Ability of "Backward" Children,' *Brit. Journ. of Psychol.*, 1911, 4, 311.

intensively. Degree of attention is measured, not by the simple reaction time, but by the reciprocal of the prolongation in reaction time produced by the use of irregularly mixed preparatory intervals of widely differing lengths in place of a regularly repeated preparatory interval of two seconds.

The results lead to the conclusion that mode of stimulus is a condition of attention. At moderate intensities of stimulus, a higher degree of attention is secured with touch as a stimulus than with sound, and a higher degree with sound than with light. In the case of choice reactions, where the psychological processes involved are more complex than in the case of simple reactions, and where the subject does not know beforehand what change in stimulus to expect, degree of attention is lower than in the case of simple reactions.

In all four of the above cases, marked individual differences in degree of attention were found to exist, even among the adults. While these individual differences vary in absolute magnitude in the different cases, they remain relatively the same. If we think of the measurements made in each case as made on a different scale, we may say that the degree of attention of each individual persists unchanged in all cases. It follows that the degree of attention of any individual is conditioned in part by certain general conditions which remain constant while the type of the mental processes concerned is varied, and that it is to these general conditions, alone, that are due the individual differences in degree of attention found in the present investigation.

The variety of cases of attention used is sufficient to render it probable that these general conditions of attention are operative as a constant in all cases of attention of a given individual. Their influence is in most cases somewhat obscured because it is ordinarily impossible to obtain for different subjects sameness in the more or less specific conditions which in every case of attention act in conjunction with these general conditions. For any case of attention, then, we may write the equation,

$$A = (k, c_1, c_2, c_3, \ldots, c_n),$$

in which A is degree of attention, c_1, c_2, etc., are those conditions of attention which vary from one case of attention to another, while k is a factor which remains constant for a given individual, tending to make his attention uniformly better or worse than that of another. The presence of the factor k in this equation means that every individul has a certain power or faculty of attention, in the sense that the degree of his attention is determined in part by general conditions which remain effective in spite of variation in the specific type of the mental activity in question.

THE FACTORS AFFECTING A PERMANENT IMPRESSION DEVELOPED THROUGH REPETITION

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One of the most interesting psychological problems now confronting us is that concerned with the distribution of intervals of time between working periods. It is now recognized in certain industries that a man can do more work with less fatigue by working in spurts and then resting than by working at a slower but steady pace. Neither the psychologist nor the physiologist has had much to do with these industrial adaptations. But both knew enough in an academic way to have made some valuable prophecies in days gone by. Today both should strive for a clearer and better understanding of the principles underlying these problems, as through such knowledge should come many valuable contributions to industry, as well as to education.

The application to education looms much more important when we substitute the term 'presentation of stimuli' for 'working-period.' The whole problem of one-hour versus five-hour courses comes then under this heading. Likewise the problem of how long classes should be held is included here. And then again the manner of presentation of a subject is involved: Should a topic be taken up and finished and then another similarly treated, or should they be unfolded little by little over a considerable lapse of time. Many other such topics will occur to the reader, all of which hinge on this general subject of how stimulations can be best distributed so as to bring about the greatest permanent effect.

There are at least two general methods of approach to this field of investigation. We may first take up and study one factor, such as the interval between stimulations, and

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vary it alone while keeping all the other factors constant. This method undoubtedly gives us the most detailed information as to the value of the particular factor under study, but it often leads us to manufacture total situations far from what we should meet in every day life. And then when we have in this way discovered how each factor operates, this method will in all probability require further experimentation to discover how two or more factors will behave when united together. Over against this method is the second one where combinations of various factors as we find them in life are studied. Here we may discover just how the several factors may operate together but we may not be able to discover the laws of behavior of each one separately.

The writer believes both these lines of attack are needed. For that reason the experiment reported in the *Psychological Review* of March, 1914,[1] in which advertisements were shown at intervals of one month, has been repeated. In the new experiments different intervals of time between successive presentations have been employed. A number of interesting relations have been found through a comparison of the various details of the experiments. The writer believes that many more such relations are still to be discovered, possibly from the data now on hand, but more likely after more such experiments have been run.

The Experiment

The same set of advertisements was used here as in the experiment reported in the *Psychological Review* of March, 1914. (For a detailed description of the set see that article.) The 288 advertisements were so arranged in four groups (dummy magazines) that we had some firms advertising once, some twice, and some four times. And moreover, we had the firms divided so that an equal number used respectively quarter-page advertisements, half-page advertisements, and one-page advertisements.

[1] E. K. Strong, Jr., 'The Effect of Size of Advertisements and Frequency of their Presentation,' *Psychol. Rev.*, 21, March, 1914.

Comparisons can consequently be made between any of the following propositions:

Firms using	full pages	and advertising	4	times
"	"	"	2	"
"	"	"	1	time
"	half pages	"	4	times
"	"	"	2	"
"	"	"	1	time
"	quarter pages	"	4	times
"	"	"	2	"
"	"	"	1	time

In these experiments the sheets, on which all the advertisements were pasted, were given to the persons being tested and they were instructed to look them through at their leisure. It was suggested that they look them through in the same way that they turn the pages of an advertising section of any magazine, looking at what interested them and ignoring the rest. In each case they were timed by a stop-watch.

In what will be referred to as Experiment I. the four dummy magazines were looked through by the subjects at their leisure, *all at the same sitting*. Four weeks later they were tested by the recognition method as to what firms they had seen in the four dummy magazines.

Experiment II. differs from the above in that an interval of *one day* occurred between the perusal of each of the four dummy magazines. The test followed here also four weeks after the first set of advertisements was seen.

Experiment III. differs from Experiment II. in that an interval of *one week* intervened between each set of advertisements instead of one day.

Experiment IV. differs from the above (1) in that the interval of time between the four dummy magazines was *one month*, and (2) in that the *test occurred sixteen weeks* after the first set was seen. This is the experiment reported in the March, 1914, *Psychological Review*.

The number of individuals who served as subjects in these four experiments was as follows:

Experiment	I.	6	men and	12	women, or a total of	18
"	II.	10	"	15	"	"	25
"	III.	12	"	10	"	"	22
"	IV.	10	"	11	"	"	21

(As there were 24 firms which advertised once in ¼-page, ½-page, and 1-page space, and 12 firms which advertised twice and 12 firms which advertised four times in each of these sized advertisements, the probable errors are determined on the basis of from 12 × 18 cases (the minimum) to 24 × 25 cases (the maximum). Such a large number of cases gives us very low probable errors, already indicated in the March, 1914, article. It has not seemed worth while to report the probable errors of Experiments I., II., and III. for this reason.)

Results

1. *The Results from Experiments I., II., III., and IV*

Table I. gives us the results from the first three experiments. They are also presented in Plate I., where the data for the three different sized advertisements are averaged together. The results show very clearly that an interval of one week between the successive presentations of a firm's advertisements gives a greater permanent impression at the end of the month than if the advertisements are seen on successive days at the commencement of the month, or if they are seen all at the same sitting.

At least four factors enter into this final determination. First, the length of time between seeing a particular advertisement and the day of the test. Second, the interval of time between the successive exposures, already pointed out. Third, the total number of advertisements seen at one time (42 pages of advertising seen at one time in Experiments II., III., and IV. and 168 pages in Experiment I.). And fourth, the effect of the size of the advertisement. Each of these factors will be discussed presently, but it is well to have them in mind in considering the gross results of these experiments.

In order now to compare the data in Table I., where the

TABLE I

SHOWING THE PER CENT. OF FIRMS REMEMBERED PER READER FOR EACH OF THE NINE COMBINATIONS OF SPACE AND FREQUENCY

Test made four weeks after first dummy magazine was seen.

1. *When the 4 Dummy Magazines Were Seen One Right After the Other (Experiment I.)*

Frequency	Size of Ads.			Av. 3 Sizes
	¼ Page	½ Page	1 Page	
1 time	3.4	5.0	10.1	6.2
2 times	4.6	8.2	15.9	9.6
4 times	4.8	12.4	16.6	11.3

2. *When the 4 Dummy Magazines Were Seen 1 Day Apart (Experiment II.)*

Frequency	Size of Ads.			Av. 3 Sizes
	¼ Page	½ Page	1 Page	
1 time	4.5	8.5	13.2	9.4
2 times	8.0	11.6	20.7	13.4
3 times	12.5	15.6	20.4	16.2

3. *When the 4 Dummy Magazines Were Seen 1 Week Apart (Experiment III.)*

Frequency	Size of Ads.			Av. 3 Sizes
	¼ Page	½ Page	1 Page	
1 time	3.7	9.8	19.9	11.1
2 times	9.0	13.7	23.9	15.5
3 times	10.5	26.8	29.2	22.2

test followed one month after the first advertisements were seen, with the data from Experiment IV., where the test followed four months after the first advertisements were seen, it is necessary to make allowance for the amount that would be normally forgotten in the interval between one month and four months. A separate series of three experiments were run for that purpose and the results are shown in Table II.

TABLE II

SHOWING THE EFFECT OF TIME UPON REMEMBERING ADVERTISEMENTS

The average firm was remembered immediately after by 50.5 per cent. of the readers.
" " " " " one month after by 14.9 per cent. " " "
" " " " " four months after by 8.6 per cent. " " "

Evidently, then, in the interval between one month and four months there has been a decrease in memory from 14.9 per cent. to 8.6 per cent. Expressed in a ratio the relative amounts remembered after one month and four months are 100: 58.

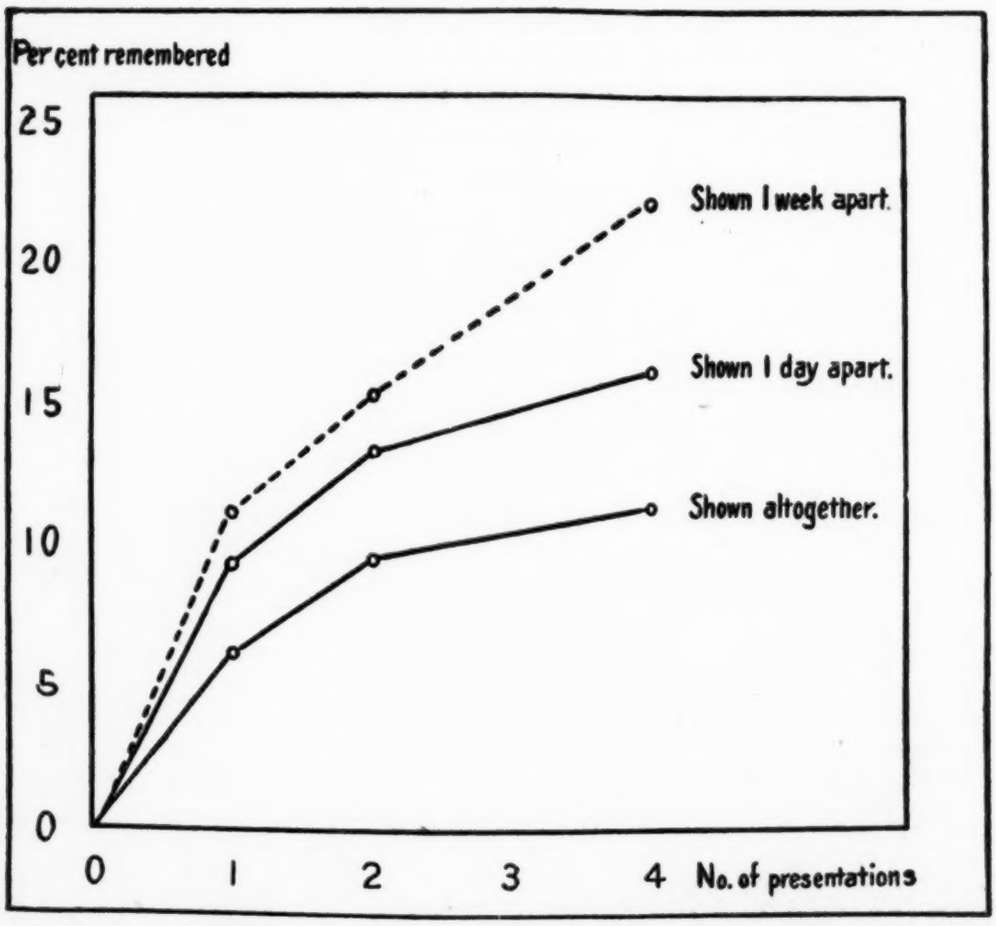

PLATE I. Showing the per cent. of readers who remember a firm when one, two, and four advertisements are shown (1) altogether, (2) at intervals of 1 day, and (3) at intervals of 1 week: the test following 4 weeks after the first set was seen.

When the averages of the data in Table I. have been multiplied by this decimal (0.58) we have the figures as given in the first column of Table III.

This decimal (0.58) is apparently a trifle too high, for when the value of one presentation in Experiment III. as given in Table I. is multiplied by it, we get a value of 6.4. Now, if anything, this value in Experiment III. should be slightly less than that in Experiment IV. (*i. e.*, 6.1 per cent.).

In the former there were 15 concerns whose advertisements appeared only once, say on January 1, 21 other concerns whose advertisements appeared on January 8, 15 more on January 15, and 21 more on January 22. The lengths of time between

TABLE III

SHOWING THE PER CENT. OF FIRMS REMEMBERED PER READER FOR EACH OF THE NINE COMBINATIONS OF SPACE AND FREQUENCY

Test made 16 weeks after first dummy magazine was seen. (Data in Experiments I., II., and III. taken from Table I. and multiplied by the decimals 0.58, 0.47, or an average of the two.)

	Times Ads. Were Seen	Correction Decimals			Ratios
		× 0.58	× 0.47	Av. of Two	
1. When the four dummy magazines were seen one right after the other	1	3.6	3.0	3.3	1.00
	2	5.6	4.7	5.2	1.58
	4	6.5	5.4	6.0	1.82
2. When the four dummy magazines were seen 1 day apart	1	5.1	4.6	4.9	1.00
	2	7.8	6.6	7.2	1.47
	4	9.4	7.9	8.7	1.78
3. When the four dummy magazines were seen 1 week apart	1	6.4	5.4	5.9	1.00
	2	9.0	7.3	8.2	1.39
	4	12.8	10.9	11.9	2.02
No correction decimal, data actually obtained with 16 week interval.					
4. When the four dummy magazines were seen 1 month apart	1	6.1	6.1	6.1	1.00
	2	7.3	7.3	7.3	1.20
	4	9.2	9.2	9.2	1.51

seeing the advertisements and the test for these four groups were then, respectively, 16 weeks, 15 weeks, 14 weeks, and 13 weeks. Now in the latter the four groups were shown on January 1, January 29, February 26, and March 26. And the lengths of time between seeing the advertisements and the test for these groups were, respectively, 16 weeks, 12 weeks, 8 weeks, and 4 weeks. No other factor, besides this one of difference in interval of time, enters into the two experiments. Hence the figure in Experiment IV. should be higher than that in Experiment III. for concerns showing one advertisement, because there was a shorter interval of time on the

average between the exposure and the test in Experiment IV. than in Experiment III. The average length of time between exposure and test per advertisement in the former was 9.7 weeks, while in the latter it was 14.4 weeks. Interpolating from our data in Table II. we should expect, if we express the percentage remembered in Experiment IV. as 100, that the percentage remembered in Experiment III. would be 88. This would mean that one presentation in Experiment III. should have a value of 5.4 (6.1 × 0.88) instead of 6.4.

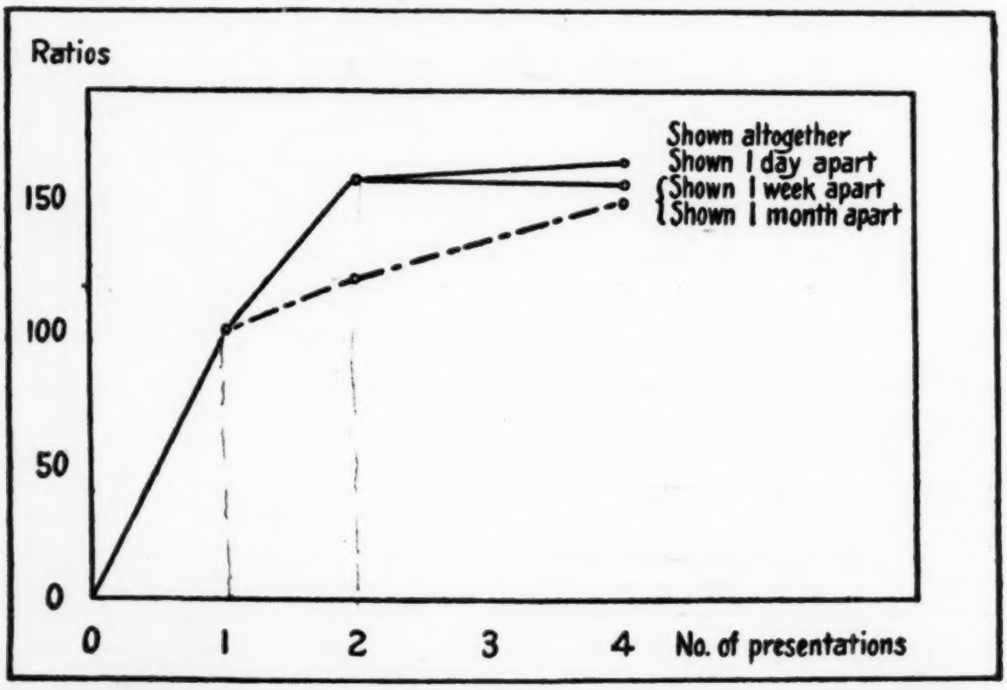

PLATE II. Showing the per cent. of readers who remember a firm when one, two, and four advertisements are shown (1) altogether, (2) at intervals of 1 day, (3) at intervals of 1 week, and (4) at intervals of 1 month: the test following 16 weeks after the first set was seen.

If 5.4 is the correct value then our correction decimal should not be 0.58 but rather 0.47. The latter decimal would give us the figures in the second column of Table III.

An average of the two sets of data thus worked out gives us the third column in Table III. These figures are plotted in Plate II. It is clear from the table and plate that a greater permanent impression is made four months later, when a firm advertises for four successive weeks and then stops, than

if it advertises once a month during that period, or on four successive days, or advertises in four different magazines which are seen by a reader at the same time. It is also clear that the latter procedure is the least efficient of the four distributions. But from the data and the plate it is not at all clear whether advertising on four successive days or on four successive months will give the greater returns. The latter distribution is favored by the data here. But the probable errors of our determination are greater than the differences.

The writer was so certain that intervals of one day would be found superior to those of a few minutes, a week, or a month, that he stated at one time that "of all intervals between successive repetitions that of a day's length will give us our maximum results."[1] This conclusion was reached on the basis of the work of Starch, Pyle, Ebbinghaus, etc. But it is clear that, when there is a long interval of time between exposure and the final test, presentations on successive days are not so effective as when they occur at intervals of one week.

refer to page 322

2. *The Effect of Different Intervals of Time between Seeing an Advertisement and its Recognition*

Table II. presents what data there ~~are~~ Bare in existence on this particular point. The curve of forgetting for recognition memory (in the case of unconnected words) has already been shown to approximate the curve as given us by Ebbinghaus for recall memory.[2] We would naturally suppose then that the great bulk of the loss in what is remembered immediately after seeing the advertisements and one month later occurs during the first two days. If this is the case the amount forgotten in the interval between two days after the original presentation and the test bears a nearly constant ratio to the interval of time. Various calculations of the writer on this

[1] E. K. Strong, Jr., 'Two Factors which Influence Economical Learning,' *Jour. Philos., Psychol. and Sci. Methods*, XI., Feb. 26, 1914.

[2] E. K. Strong, Jr., 'The Effect of Time-Interval upon Recognition Memory,' *Psychol. Rev.*, XX., Sept., 1913.

basis in different experiments have checked up with directly obtained data, as closely as could be expected.

3. *The Effect of Different Intervals of Time between the Successive Presentations of Advertisements when Tested at a Considerable Time Later*

If the data in Table III. are expressed in ratios in terms of the value of advertisements seen once, we have the ratios given in column four of that table. These are plotted in

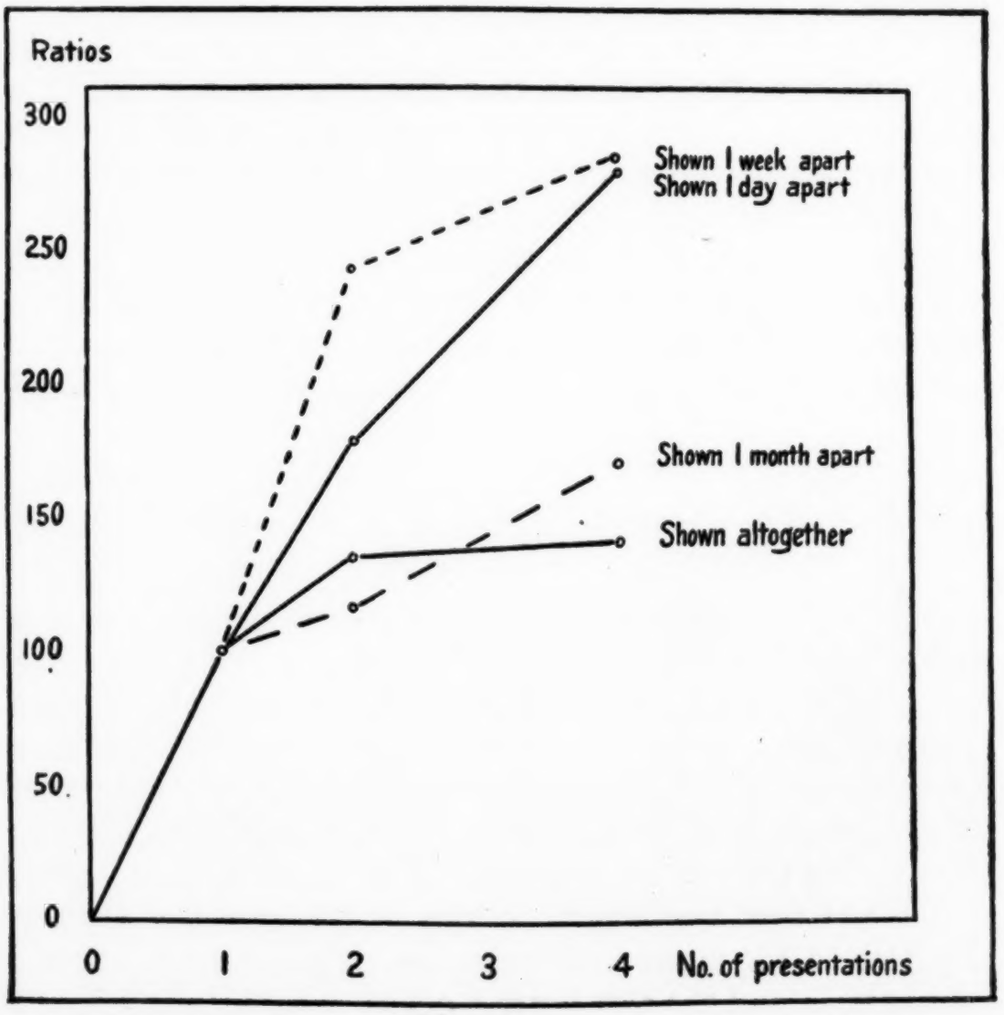

PLATE III. Showing the cumulative effect of two and four repetitions over one with the four methods of distribution. The value of one presentation is in each case called 100 and the other values are expressed in ratios of this value.

Plate III. With one exception, *i. e.*, the effect of four presentations a week apart, the curves show very clearly that the

shorter the interval of time between the successive presentations the greater is the cumulative effect.

But the relation between 'intervals of time' and 'permanent impression' from successive stimulations is not so simple as would appear from Plate III. In Plates IV., V., and VI. are shown the same relations as in Plate III. but here restricted, respectively, to ¼-page advertisements, ½-page advertisements and 1-page advertisements, instead of giving an average of all three sized advertisements.

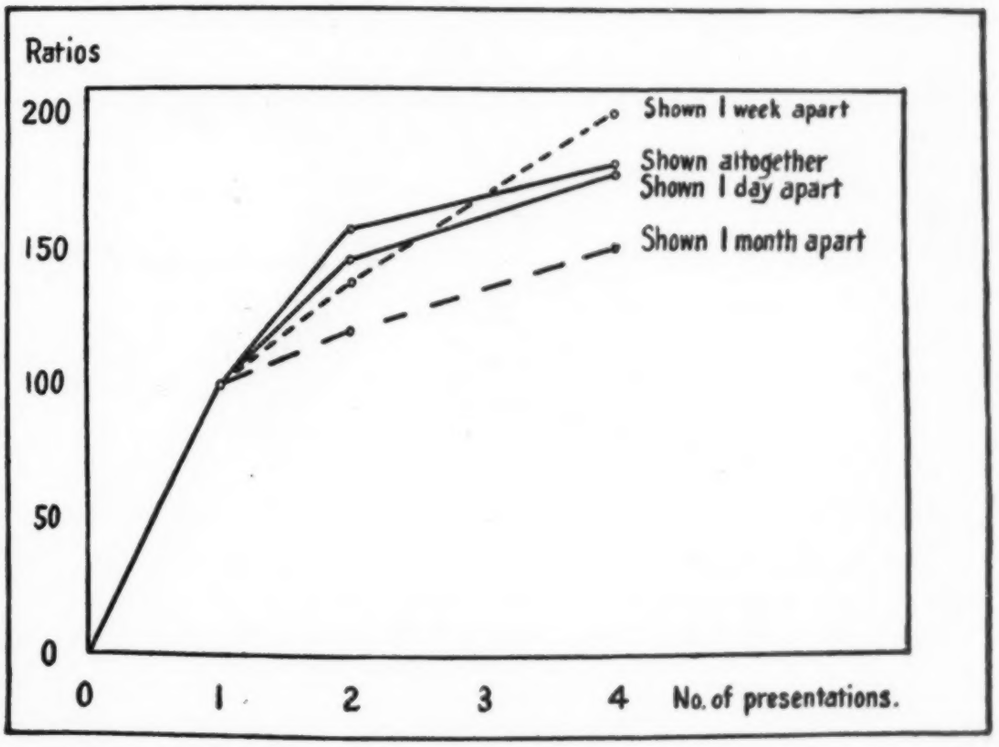

PLATE IV. With ¼-page advertisements. Showing the cumulative effect of two and four repetitions over one with the four methods of distribution. The value of one presentation is in each case called 100 and the other values are expressed in ratios of this value.

The first fact that appears from a study of these plates is that a relatively greater effect is made by the first presentation of a large advertisement than by a small advertisement, as compared to the effect made by two or four presentations. Or possibly, the relation should be expressed as follows: A second presentation adds relatively more to the permanent impression when the advertisement is small than when it is large.

A second fact that appears is that repetition within a few minutes has relatively little effect upon ¼-page advertisements as compared with repetition at longer intervals of time, but that it has much greater effect with ½-page advertisements, and has the greatest effect of all our intervals of

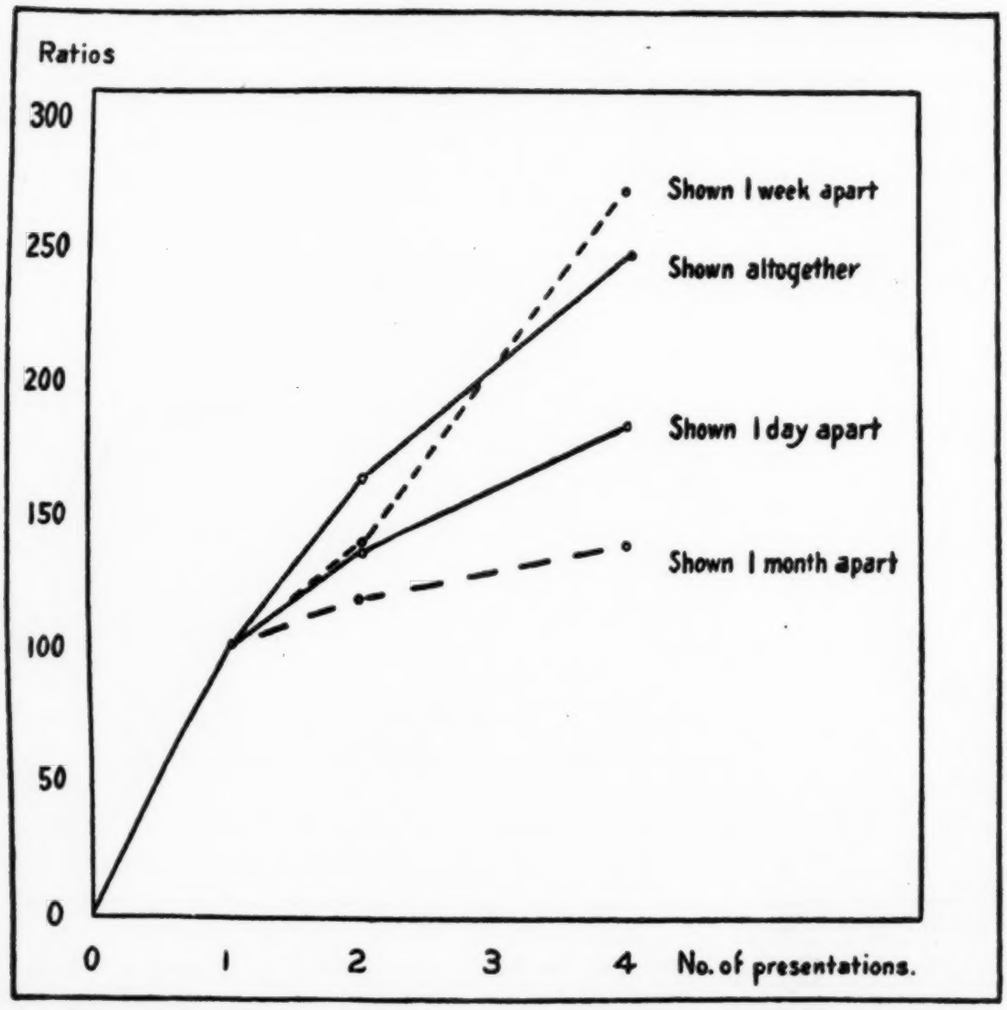

PLATE V. With ½-page advertisements. Showing the cumulative effect of two and four repetitions over one with the four methods of distribution. The value of one presentation is in each case called 100 and the other values are expressed in ratios of this value.

time upon 1-page advertisements. On the other hand, repetition at intervals of one month has (with one exception) the least effect of all the intervals regardless of the size of the advertisements.

Our general conclusion as to the effect of different intervals

of time upon the permanent impression, *i. e.*, the shorter the interval of time between the successive presentations the greater is the cumulative effect, holds exactly for 1-page advertisements and nearly so for ½-page advertisements, but not at all for ¼-page advertisements.

The results obtained here support previous work as to the greater impression made by the first stimulation than by any succeeding one. But they do not entirely support Smith's[1] extreme statement that "the first repetition is undoubtedly the best, *i. e.*, more is learned by it than by any other repeti-

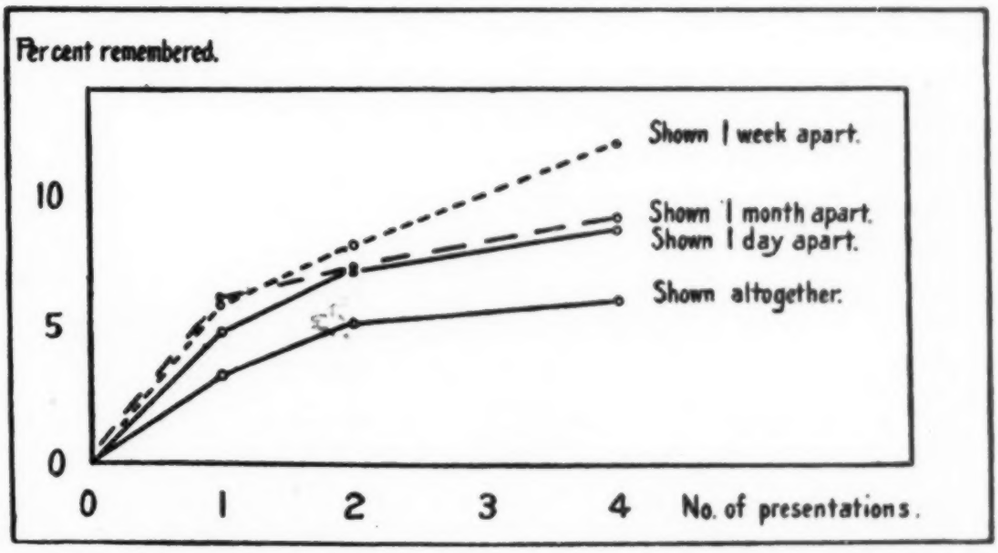

PLATE VI. With 1-page advertisements. Showing the cumulative effect of two and four repetitions over one with the four methods of distribution. The value of one presentation is in each case called 100 and the other values are expressed in ratios of this value.

tion, or, in fact, by all the other repetitions put together." In his experiment, as in these, here, the subjects were not directed to learn but simply to notice what was presented to them. Smith's conclusion is correct as far as our data go with the full-page advertisements, for here three more repetitions have not doubled the effect of the first one. The same holds true in the case of ½-page advertisements when the intervals were either one month or one day apart, and in the case of ¼-page advertisements when the intervals were either

[1] W. G. Smith, 'The Place of Repetition in Memory,' *Psychol. Rev.*, III., 1896, 21–31.

one month or a few minutes apart. But in four cases, the three additional repetitions more than doubled the effect of the first repetition.

4. *The Effect of Seeing Varying Totals of Advertisements at One Time*

The writer has already shown at some length that as you increase the number of advertisements which are seen at any one time, you decrease the effectiveness of the permanent impression from any one of them.[1] "Increasing the number of advertising pages from 42 to 168 resulted in a decrease in the percentage remembered among the 42 pages from 14.9 per cent. to 7.8 per cent. That means that the situation in which but 42 pages are glanced at allows a 91 per cent. greater impression to be made (from identically the same advertisements) than that which can be made when 168 pages are glanced at."

5. *The Effect of Different Sized Advertisements upon the Permanent Impression*

If we average the effect of the one, two, and four presentations together, as given in Table I. and the last part of Table II. in the March, 1914, article, we have the following as the value of ¼-page, ½-page, and 1-page advertisements, respectively:

	¼-page	½-page	1-page
When shown all together	4.3	8.5	14.2
When shown 1 day apart	8.3	11.9	18.1
When shown 1 week apart	7.7	16.8	24.3
When shown 1 month apart	4.9	7.0	10.7
Average	6.3	11.1	16.8

If these are expressed in ratios, we have:

	¼-page	½-page	1-page
When shown all together	100	198	330
When shown 1 day apart	100	143	218
When shown 1 week apart	100	218	316
When shown 1 month apart	100	143	218
Average	100	176	267

[1] *Op. cit.* (see note 2).

These ratios are higher than those previously reported in this particular experiment, which were 100: 141: 215, but are very similar to ratios obtained in other experiments, as given in the March, 1914, article, *i. e.*, 100: 166: 241 as the average of four experiments.

More recently the attention-value of various sized advertisements in the *Saturday Evening Post* has been ascertained.[1] In this case 285 individuals were canvassed and from this

TABLE IV

THE ATTENTION-VALUE OF DIFFERENT SIZED ADVERTISEMENTS IN THE *Saturday Evening Post*

Size of Advertisements	Supposed Value, *i. e.*, Percentage of Size of a 1-Page Ad.	Value as Obtained in this Experiment Expressed in Terms of a 1-Page Ad.	Theoretical Value, *i. e.*, Based on the Square Root Law
2 page	200%	149%	141%
1 page	100	100	100
½ page	50	81	71
2 cols. × 8″	31	70	56
2 " × 6½	25	57	50
2 " × 5	21	44	45
2 " × 4	17	41	40
1 " × 7	14	32	37
1 " × 5½	11	32	34
1 " × 4½	9	25	31
1 " × 3½	7	28	27
1 " × 2½	5	21	23
1 " × 1½	3	25	18

number 90 were found who had handled that week's edition. Of these 88 were tested by the recognition method as to what advertisements they had seen in the last number of the *Saturday Evening Post.* In Table IV. are given the results as they bear on the value of space. They are expressed in terms of ratios of the attention-value of a full-page. It is clear from this table (based on 113 advertisements) that attention-value does not vary directly with the size of the space, but does vary very closely with the square-root of the size of the space. The figures, indeed, correspond as closely as one could expect.

Generally speaking our previous conclusion is correct,

[1] E. K. Strong, Jr., 'A Study of the *Saturday Evening Post*,' Association of National Advertising Managers' Research Bulletin No. 7, Mar., 1914.

i. e., 'that the attention-value of space increases approximately as the square-root of the increase in area and not directly with the increase in area.' This is exactly so in some cases: in others the increase in attention-value is in excess of the square-root ratio by as much as 25 to 30 per cent.

6. *Relative Strength of these Four Factors*

Undoubtedly the third factor—the effect of varying numbers of advertisements seen at one time—is the most important factor as affecting the total impression. An increase from 42 to 168 pages of advertising results in a decrease of 48 per cent. in the effectiveness of the impression from any one advertisement. The difference as shown in Plate I. between the data of Experiments I. and II. is due to this factor of seeing 42 advertisements at one time versus 168 advertisements at one time. It must be due to this factor, as the difference in the number of days between presentation and testing is too slight to account for any appreciable difference in the amount remembered. Now we have seen in Section 3 that repetition within a few minutes has a greater effect than at longer intervals with 1-page advertisements. If this is actually true, then it would seem that if we had shown the four dummy magazines in Experiment I. at intervals of one hour instead of one right after the other, there would have been a greater permanent impression made with the 1-page advertisements than by any of the other distributions used here. It is, however, possible that the greater cumulative effect from seeing the advertisements within a few minutes is due to the fact that the first impression made in Experiment I. was very slight. This conception is suggested by the fact that all the types of distribution have a greater cumulative effect upon small advertisements than large ones. If this latter conception is true then our above prophecy is not necessarily correct. The matter can only be settled by actual experimentation.

As was shown in the March, 1914, article, the same total amount of space is more effective when used in large amounts than when used in small amounts but more frequently.

Table V. shows this fact very clearly. Here the value of a ¼-page advertisement shown once is called 100. In all but two of the thirty-two cases the large space seen less often is more effective than the smaller space seen often.

TABLE V

SHOWING THE VALUE OF THE NINE COMBINATIONS OF ¼-PAGE, ½-PAGE, AND 1-PAGE SPACE PRESENTED ONCE, TWICE, AND FOUR TIMES

Each value is stated in terms of the value of a ¼-page advertisement shown once.

Size of Advertisement	Number of Presentations	Shown Altogether	Shown One Day Apart	Shown One Week Apart	Shown One Month Apart
¼-page ad.....	1 time	100	100	100	100
¼-page ad.....	2 times	135	178	243	116
½-page ad.....	1 time	147	189	259	155
¼-page ad.....	4 times	141	278	284	171
½-page ad.....	2 times	241	258	370	184
1-page ad.....	1 time	297	293	538	229
½-page ad.....	4 times	365	347	724	216
1-page ad.....	2 times	468	460	646	276
1-page ad.....	4 times	488	452	789	342

Possibly the relationship is something as follows: When an advertisement is seen it arouses many associations more or less well established. A large advertisement on the average arouses many more such associations than a small advertisement. But the increase in number is never in proportion to the increase in space. All these associations tend to be welded into a complex giving us the new conception desired by the advertiser. We have then finally as resulting from the impression, a more or less clearly formed new conception—a sort of higher-order association formed from the old associations. The more associations which were aroused the better formed is the complex. When the second advertisement is seen it again arouses this complex and adds to it one or more new associations. But we must judge from our data that the higher-order association is more readily developed when the many old associations (with possibly some new ones) are all aroused at the same time than when some are aroused at one time and then later aroused again in a different setting. (It

should be remembered here that the repetition consisted in these experiments not of the same original advertisements but of new ones which were more or less similar to the first advertisement displayed.) It is very likely true that in the case of the small advertisement, the total impression from the first advertisement varied much more from that of the second advertisement than when large space was employed. There would be in this case less overlapping of the associations in the repetition of the small advertisements and more emphasis upon new associations (new to the complex) than in the case of the large advertisements.

At first thought it would seem as though the results in Sections 5 and 6 could not be harmonized. First we see that attention-value of large advertisements is never proportional to their size. Hence the argument arises, use small space for effective advertising. Then we see in Section 6 that a full-page advertisement is more effective than four quarter-page advertisements. Now the argument must surely be, use large space for effective advertising. Can both be correct? To the writer it seems that common advertising practice supports both as correct. When the advertisement is expected practically to complete the sale, as in mail-order advertising, we find generally small space is employed. Mail-order houses are the only advertisers who are in a position really to know the effect of their advertising. The fact that they ordinarily use small space seems to demonstrate that small space is more efficient than large space. And such advertisers constantly affirm that this is so. Now, on the other hand, when it is not expected that the first advertisement will sell the goods but that only after many such advertisements have been run will the advertiser reap the effect of the advertising, as in advertising pianos, autos, etc., we find that large space is used almost altogether. It is doubtful if any such advertiser has ever been able really to demonstrate the value of this style of publicity. But certainly at the present time the majority of such campaigns are carried on in this way. We should conclude, then, by saying that in Section 5 is given the relative attention-value

of advertisements of varied sizes when there is but one presentation, whereas in Section 6 there is given the relative attention-value of advertisements of various sizes when the factor of repetition is taken into account. In the first case the attention-value is measured after one repetition; in the second case after many repetitions.[1]

Conclusion

1. A greater permanent impression is made (*i. e.*, 16 weeks after the first presentation) when a firm advertises for four successive weeks and then stops, than if it advertises once a month during that period, or on four successive days, or advertises in four different magazines which are seen by a reader at the same time. The last form of distribution is the least efficient. The second and third are approximately equal.

2. The permanent impression, after the first two days, apparently dies out gradually and in a direct ratio to the total length of time which has elapsed.

3. It would seem that the greater the initial impression the less will be the added impression from successive repetitions.

4. It would also seem that the greater the initial impression (*i. e.*, one-page advertisements here) the more does repetition within a few minutes affect the total impression as compared with repetition at longer intervals such as a day or longer.

5. As you increase the number of advertisements which are seen at any one time you decrease the effectiveness of the permanent impression from any one of them.

[1] The factors discussed in this paper are, of course, not the only ones that enter into the question as to what size of space will be most effective. Large space may be very ineffective from the advertising standpoint but very effective from the manufacturing standpoint. That is, it may cost more per dollar spent for advertisers to use large space than small space, but it may save far more through producing a larger total of sales, thus cutting down the cost of production. It is probable that economic factors, such as this, now dominate the reasons for selecting one size of advertisement over that of other sizes. But when small differences in the cost of salesmanship will seriously affect the entire business the psychological factors considered in this section are all important. See W. A. Shryer, 'Some Mail Order Weaknesses and their Cure, Advertising and Selling,' May, 1915.

6. The value of space in advertising as affecting permanent impressions increases approximately as the square-root of the increase in area, or sometimes at a somewhat greater rate.

7. The same total amount of space is more effective when used in large amounts less often, than when used in small amounts but more frequently.

INCIDENTAL PERCEPTION

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The aim of this paper is to study incidental perception of time, size and weight and to make some comparisons between incidental and purposive perception of time and size.

EXPERIMENT I

Forty men of Juniata College and Academy were each given three successive tests for estimating a minute interval of time. All, save a few day students, were tested on the same evening during study hours, when no chance was offered the prospective subjects to be apprised of the nature of the test by those already tested. In the first test the subject was told to read silently from a certain editorial page of the *Saturday Evening Post.* At the end of a minute he was interrupted by the request to guess how long he had been reading. Then he was given a poem entitled 'Two Chums,' and was told to read it carefully and to state when he had read one minute. Finally he was told to estimate, while unemployed, a minute from the signal 'go.' He was cautioned not to count and not to count his breaths. A stop watch was used in this and the following experiments. The writer was the experimenter.

The average time estimated in the first test was 1 min. 23 sec., M.V. 24.6 sec.; in the second test, the average was 44.3 sec., M.V. 12.6; and in the third, 37.9 sec., M.V. 15.7. The average error for the first test was 24.2 sec. and the constant error, + 22 sec. The highest estimate was 2 min. 30 sec.; lowest 45 sec. Nine estimated at 2 min., 6 at 1 min. 30 sec. Twelve estimated correctly; 25 estimated above a minute; 3, below. In the second test the A.E. was 19.8 and C.E. −15.7. The highest estimate was at 90 sec.; lowest, 19. Two cases were correct, 5 too high, and 33 too low.

In the third test the A.E. was 23.8; C.E. −22.1. No cases were correct, 3 were too high, and 37 too low. Thirty-four made higher estimates when they were asked to estimate how long they had read (1st test) than they made when they were asked to designate when they had read for a minute (2d test); two made the same, four made lower estimates. Following are some random samplings of the three estimates.

Individual	1st Test	2d Test	3d Test
1	1 min. 23 sec.	35 sec.	17 sec.
2	2 min. 10 sec.	27 sec.	35 sec.
3	1 min. 15 sec.	65 sec.	37 sec.
4	1 min.	36 sec.	32 sec.
5	50 sec.	52 sec.	49 sec.
6	45 sec.	81 sec.	42 sec.
7	1 min. 15 sec.	27 sec.	40 sec.
8	1 min. 5 sec.	47 sec.	57 sec.

The large C.E.'s and the small difference between the C.E.'s and A.E.'s indicate strong overestimation and that nearly all errors made were in this direction. The variation among the three tests by the same performer as well as the variation among the performers in the same test was rather pronounced. The M.V.'s were about one third the averages. There was a slightly greater tendency to overestimation in incidental perception than in purposive perception, but the overestimation in the case of incidental perception of filled time and that of purposive perception of empty time were about the same. Although 37 of the 40 in the third test against 25 in the first test overestimated, the large number (13) who estimated at 1 minute may help explain this discrepancy.

The subjects were asked for introspections in the last test. Most said they could offer no explanation of how they estimated; a few gave brief introspections; "I measured in steps;" "I felt myself measuring" (29); "Just guessed" (39); "Felt rhythm of breathing" (57); "Grouped in series of 10" (38); "Allowed a little for grace" (43); "Imagined I heard and saw a watch" (22); Four imagined the second hand moving (29, 20, 32, 28). The numbers in parenthesis indicate the respective estimates. Apparently these devices were of no advantage.

Experiment II

Forty-six normal-school girls were given group tests. At the end of their logic courses the writer read from the introductory chapter of Creighton's 'Logic,' presumably to present some facts in logic. At the end of one minute he asked them to write down a 'precise estimate' of how long he had read. Then he resumed reading for another minute and again asked for their estimates. Then they were told to estimate the time between the signals "Go" and "Halt."

The average estimate for the first test was 1 min. 44.1 sec. with a M.V. of 44.5 sec.; second test, 1 min. 29.3 sec., M.V. 38.4; third test, 1 min. 39.2 sec., M.V. 42.6. The ratio of the highest variation to the lowest was practically 2 to 1 (first test 196–79 sec.; second 151–79 sec.; third 121–71 sec.). The M.V. is between ½ and ⅓ the average. Thus individual difference is tremendous and the women are a little more variable than were the men in Ex. I., though the tests are slightly different.

In the first test 7 estimated correctly, 8 underestimated and the rest, 31, overestimated a minute with a C.E. of + 39.8 sec. and A.E. of 46.2 sec. In the second test 2 estimated correctly, 16 underestimated and the remaining 28 overestimated, with a C.E. of + 30.8, A.E. 38.6. In the third test 4 estimated correctly, 10 underestimated and the remaining 32 overestimated. The C.E. + 39.1, A.E. 47.7.

Not one individual estimated the minute of one length in the three successive tests and only four got any two estimates the same. The correlation of first and second tests gives + .509, second and third + .338, first and third + .278. In the second test 34 gave smaller estimates than in first test; 10, larger; and 2, the same. In the third test 15 gave smaller estimates than they did in the second; 28 gave larger, and 3 same. Also 24 gave smaller estimates in the third test than in the first, 20 gave larger and 2 gave the same. These small correlations suggest a wide variation in the estimation of a minute of time under different conditions by each individual.

Therefore there are very wide individual differences as to

the influence of different conditions in determining the estimates, and there is a strong tendency at overestimation of time. As was also shown by experiment I., the overestimation of incidental perception of time was slightly greater than it was by purposive perception during employment of the subject; but the incidental estimate in both experiments when the subject was unemployed was about the same as when estimating empty time purposely perceived.

Assuming that employment by listening to material read factors the same in estimating time as employment by reading, the C.E.'s by the women, Ex. II., show a much greater overestimation of a minute than by the men, Ex. I. This assumption, however, deserves experimental evidence.

Accidentally the writer, in testing 20 girls of a class of psychology with the same experiment, said, in giving the directions: "Estimate the time during which I was reading, in *minutes and seconds*." The influence of suggestion was rather pronounced. Out of the total 60 estimates all except one was above a minute and that one was just one minute. The records are given below in full along with twenty records randomly selected from those not exposed to the suggestion.

First Test		Second Test		Third Test	
With Suggestion	Without Suggestion	With Suggestion	Without Suggestion	With Suggestion	Without Suggestion
220	75	195	80	130	100
210	65	185	23	160	28
182	80	240	90	180	45
180	30	240	120	110	80
150	180	210	148	60	120
200	70	250	150	180	125
130	120	120	120	90	150
80	180	65	160	65	210
150	59	180	130	90	140
300	62	240	61	120	62
180	25	150	120	100	220
150	85	110	60	70	160
255	60	285	50	170	80
180	130	210	100	80	120
150	120	180	100	180	60
240	180	300	120	120	90
210	120	245	90	180	38
180	45	210	75	90	180
160	60	210	70	80	45
180	150	240	120	120	90
184.3	94.8	203.2	99.3	113.7	107.1

While the suggestion is not so pronounced for the third test the average estimate in the first and second tests is about twice as great with the suggestion as it is without the suggestion. These records emphasize how readily the results of an experiment may be vitiated by suggestion from the experimenter.

Experiment III

On Monday following a Saturday evening basket-ball game 100 students—68 men and 32 women—of Juniata College and Academy who had attended the game, were asked in their respective classes, at the same hour, to estimate how long they thought the teams had played before "time out" was called. At the time referred to a player had received a rather serious injury, 6 minutes 15 seconds, after the game had started. The average estimate by the hundred students was 11 min. 38 sec. By the men it was 10 min. 7 sec. with a M.V. of 3 min. 4 sec., an A.E. from the standard (6 min. 15 sec.) of 4 min. 28 sec. and a C.E. of + 3 min. 52 sec. Only 9 of the 68 men underestimated the time. By the women the average was 14 min. 54 sec. with M.V. of 7 min. 50 sec., an A.E. of 7 min. 46 sec. and a C.E. of + 6 min. 54 sec. Only 11 women underestimated and their estimate was at 5. All the other women estimated in whole minutes and all but 10 of the 32 estimated in some multiple of five; whereas 6 of the men estimated in minutes and seconds and only 25 of the 68 men estimated in some multiple of 5. The highest estimate by the men was 22 min.; the lowest, 45 sec. The highest estimate by the women was 25 min., the lowest, 5 min. Fifty-six per cent. of the men's estimates were between 7 and 13; 34 per cent. of the women's records were between these limits, and 62.5 per cent. of the women's exceeded the average of the men's. (Delayed report may have been responsible for some of the error.)

Thus by incidental perception 6 min. 15 sec. was overestimated by about 80 per cent. of the cases and more by women than by the men. Likewise the women varied more than did the men. For both, the general tendency at over-

estimating time and the predominance of the men, in this respect, over the women is in line with the findings of Yerkes and F. M. Urban[1] in purposive perception and of Myers[2] in incidental perception of time. For example the writer found that of 70 school children, 36 boys estimated two and one half minutes during which they had been employed, at an average of 5.4 minutes and 34 girls at an average of 6.4 minutes.

Experiment IV (*a*)

In the center of each of two white cardboards 3 × 3 inches, a circle was drawn. One circle represented the outer edge of a cent, the other, that of a half dollar. Forty men and forty women of Juniata College and Academy were tested in their respective rooms during study hours in one evening. Mrs. Myers was experimenter for the women and the writer for the men. Two and three students were tested at the same time. One of the circles was presented to each subject who was asked to state on paper whether or not the circle appeared to be in the center of the square. After the answer was written the experimenter removed the circles from view and presented in a visiting way, some photographs of mutual interest to experimenter and subject, for 2 minutes. Then he asked the subjects to write down the number of the circle which they would select from a cardboard on which were drawn a large number of circles as equal in size to the circle previously presented. Among these were circles representing the exact size of the respective coins and the circles numbered from 9 to 29; no reference to coins was made. Then the other circle was presented to the subject, who was told he would have a few seconds to look at it when he would be asked to select this same circle from among those on the cardboard. One minute intervened between the time the circle was recovered and the time the cardboard of circles

[1] R. M. Yerkes and F. M. Urban, 'Time Estimation in Its Relation to Sex, Ages and Psychological Rhythm,' *Harvard Studies*, II., 1906, pp. 405–30. See also Nichols Herbart, 'The Psychology of Time,' Amer. J. Psychol. 1890, 3, 453–529. He gives an exhaustive bibliography.

[2] Myers, Garry C., 'A Study in Incidental Memory,' *Archives of Psychology*, No. 26, Feb., 1913, pp. 92–93.

was presented. To one half of the subjects of each sex the cent circle was first presented and to the other half, the half-dollar circle. As the diameter of each successive circle increased by one millimeter, the difference between the numbers of the circles represents the difference in diameters of the circles. The records are given below.

CENT

Men (20)	Women (20)	M. and W. (40)
Cent Circle Shown First		
A.E. 2.0	A.E. 1.9	2.0
C.E. −.6	C.E. −1.1	−.6
5 + cases	6 + cases	11 + cases
11 − cases	10 − cases	21 − cases
4 0 cases	4 0 cases	8 0 cases
Cent Circle Shown Second		
A.E. 2.0	A.E. 1.2	1.6
C.E. −1.2	C.E. −.2	−.7
3 + cases	6 + cases	9 + cases
13 − cases	7 − cases	20 − cases
4 0 cases	7 0 cases	11 0 cases

HALF-DOLLAR CIRCLE

Half-Dollar Circle Shown First		
A.E. 2.7	A.E. 2.0	2.7
C.E. −.7	C.E. −.2	−.5
9 + cases	8 + cases	17 + cases
10 − cases	10 − cases	20 − cases
1 0 case	2 0 cases	3 0 cases
Half-Dollar Circle Shown Second		
A.E. 2.6	A.E. 1.6	2.1
C.E. −1.4	C.E. −.8	−1.1
5 + cases	4 + cases	9 + cases
14 − cases	11 − cases	25 − cases
1 0 case	5 0 cases	6 0 cases

They should read: "The 20 men to whom the cent-circle was first presented made an average error of 2.0 mm. and a C.E. of −.6 in estimating the cent circle. The records for the 20 to whom the half-dollar circle was first shown were: A.E. 2.0 and C.E. −1.2." A glance at the C.E.'s and A.E.'s, for both sexes shows little or practically no advantage to the purposive perception. It must be remembered that the size

of the circle shown first was incidentally perceived, whereas the second was purposely perceived. The women however seemed to gain more in the second test over the first than did the men, and they had a smaller A.E. than the men. The negative C.E. for all groups suggests that the sizes were underestimated. This does not agree with the findings of the writer in his 'Study of Incidental Memory' where records of tests made on several hundred subjects of various ages fall into two very distinct groups. The smaller coins, cent, nickle and dime, were almost universally underestimated whereas the larger coins—quarter, half-dollar, and dollar—were overestimated. These findings for which the writer had no satisfactory interpretation seem the more puzzling in the light of this newer data. Certainly values seem now to have played a rôle in the estimates of the sizes of the coins. It should be remembered that in the present test no reference was made to the circles representing coins when they were presented. Apparently the image of the circles first observed must have grown smaller during the interim between the perception of the circle and the selection of its equal from the cardboard. This is contrary to the central tendency of judgments as found by Hollingworth[1] and to the tendency for images to increase in size during a brief interim as found by Baldwin, Warren and Shaw.[2] The writer also found that the drawing of an estimated size of a dollar bill was almost always increased in size when corrected.[3] Evidently the standards for the measurement of a square and a circle are different. Baldwin suggests that 'circles tend to be measured by their radii but in the case of the squares the impression is that of area.'

The correlation between the estimate of the respective coins is for the first 20 women + .52, the second 20, + .65. First 20 men, + .76; second 20, + .50.

Of the women 17.5 per cent. said the circle was not in the

[1] Hollingworth, H. L., 'Central Tendency of Judgment,' *Journal of Phil., Psychology and Scientific Methods*, 1915, 7, 461–69.

[2] Baldwin, Warren and Shaw, 'Memory for Square Sizes,' *Psychological Review*, 1895, 2, 236–244.

[3] 'Incidental Memory,' pp. 41.

center of the square cardboard while 57.5 per cent. of the men said it was not. Therefore men seem more open to suggestion here than the women. However, the probable difference in the emphasis and voice modulation between the experimenters in repeating the same statements verbatim could render this difference possible. The women on the whole do better than the men and they seem to improve in their purposive perception over their incidental perception much more than the men.

Experiment V

Forty-six college men and 20 college women, and 46 boys and 20 girls of the seventh and eighth grades were asked to estimate the weight of each of two discs, one of light wood, the other of aluminum, in terms of the commonly used coins. Each disc was made to balance, on fine scales, a new nickel. The wooden disc measured in diameter 43 millimeters and in thickness 10.5 millimeters, the metal one 55 millimeters and one millimeter, respectively. The discs were given to the subject who was told that each disc represented the exact weight of one of the coins (cent, nickel, dime, quarter, half dollar, or silver dollar) and he was asked to name the coin he judged to be represented by the respective discs. No time limit was set but the subject was not allowed to shift the discs from one hand to the other. Each subject was tested individually, and no chance was afforded the subject to know anything about the test before taking it. The results are given on the following page.

In the above table one reads "Of the 46 men 8 estimated the wooden disc at 50 cents and 2 the metal disc at 50 cents; 9 the wood disc at 25 cents and 15 the metal; 12 estimated the metal disc as heavier than the wooden disc. The boys have a strong central tendency for the wooden disc at the nickel (correct), and over half of them estimated the metal disc at the dime. The tendency for the same number of men is not so pronounced as that for the boys, and falls at the dime and quarter for the respective discs. Therefore the wooden disc is better judged than the metal disc. The same is true for the girls, but not for the women. However, the

WEIGHT OF DISCS IN TERMS OF COINS

	Men (46)		Women (20)	
	Wood Disc, Cases	Metal Disc, Cases	Wood Disc, Cases	Metal Disc, Cases
$1.00				1
.50	8	2	2	1
.25	9	15	3	3
.10	15	11	9	10
.05	11	13	4	2
.01	3	5	2	3

Metal heavier than wood..... 12
Metal lighter than wood..... 12
Metal equals wood..... 22

Metal heavier than wood..... 6
Metal lighter than wood..... 7
Metal equals wood..... 7

	Boys (46)		Girls (20)	
	Wood Disc, Cases	Metal Disc, Cases	Wood Disc, Cases	Metal Disc, Cases
$1.00	1			
.50		1		
.25	5	7	1	6
.10	10	27	10	5
.05	19	4	4	4
.01	11	7	5	5

Metal heavier than wood..... 20
Metal lighter than wood..... 19
Metal equals wood..... 7

Metal heavier than wood..... 6
Metal lighter than wood..... 10
Metal equals wood..... 4

Total, Both Sexes (132)

	Wood Disc, Cases	Metal Disc, Cases
$1.00	1	1
.50	10	4
.25	18	31
.10	44	53
.05	38	23
.01	21	20

Metal heavier than wood..... 44
Metal lighter than wood..... 48
Metal equals wood..... 40

number of women tested was only 20. The number of times each disc was estimated as equal to, greater, or less than the other is about the same. Grouping all the cases regardless of sex or age, there is a marked central tendency for each disc at the dime but more for the metal than for the wood. This means that most of the subjects estimated the discs as equivalent in weight to that of the dime. Apparently then the

weight of the discs was underestimated. One may conclude perhaps that the weight of the dime is overestimated. This, however, would be contrary to the average estimation as found for the size of the dime in another study[1] where the size of all the smaller coins was underestimated. The surface area of the hand covered in judging the weight of the discs as compared with the surface covered by the respective coins is a disturbing factor, as well as the necessity of translating these unaccustomed objects into terms of the experienced objects (coins). There is no obvious sex difference but tremendous individual differences obtain.

Messenger[2] tested college students and other grown persons on their ability to estimate the number of one dollar bills and ten dollar bills respectively, that would equal the weight of a silver dollar. He found the average for all judgments 20.1 times, and the median 8 times the actual number required; Pierce,[3] an average of 15.8 times and median 4.3 times as great; MacDougall,[4] an average of 10.2 times and a median 1.3 times the actual number.

Conclusions

On the average one minute of time was overestimated by about one half. Six and a quarter minutes was doubled in the estimation of the women and overestimated by about one half by the men.

The estimation of a minute of filled time incidentally perceived was about the same as the estimation of empty time purposely perceived. There was, however, a slight gain for purposive perception of filled time over incidental perception of filled time, yet the gain is surprisingly small.

Wide individual differences obtain with a M.V. of from one fourth to almost one half the average. The women are slightly more variable than the men.

[1] Myers, G. C., 'Incidental Memory,' p. 35.

[2] Messenger, J. F., 'A Note,' Science, 15, April 25, 1902.

[3] Pierce, A. H., 'Guess on the Relative Weights of Bill and Coins,' Science, 1902, 16, 474.

[4] MacDougall, R., 'Secondary Bias in Objective Judgment,' *Psychological Review*, 1906, 13, 97–120.

In estimating the sizes of the cent and the half dollar the coins were both underestimated and the estimation after incidental perception was almost as good as that after purposive perception. The women were slightly superior to the men.

The weight of the discs in terms of coins was underestimated. Wide individual differences obtain but there was no appreciable sex difference.

The slightest suggestions by the experimenter were tremendous factors in determining the observer's performance.

Much of what has been studied herein may be classed as incidental memory. Although the writer elsewhere emphasized the untrustworthiness of incidental memory in certain cases, there was no desire to minimize its general importance. Indeed, this study strongly emphasizes the place of incidental experiences in making up the total mental content.

THE CORRELATION BETWEEN THE SEX OF OBSERVERS AND THE SEX OF PICTURES RECOGNIZED[1]

BY YO SUGISAKI AND WARNER BROWN

In the course of an experiment conducted for an entirely different purpose a large number of trials were made of the ability to recognize 20 photographs once seen when mixed with an equal number of new ones. The pictures were observed at the rate of one each half-second and the test was made as soon as the 20 pictures could be shuffled up with 20 others. The pictures were of young men and women, cut from old college annuals, and were of uniform size and color. No picture was used more than once. At one sitting the first 10 of the 20 pictures were of men, at the next sitting the first 10 were of women. A tabulation of the data, each picture having been numbered according to the order of its presentation, shows that recognition is not seriously or systematically affected by the position of the picture in the series of 20.

The persons who acted as subjects were three men and three women who were college students of slight psychological training, and 15 Japanese children attending schools in Alameda and San Francisco, California, under Japanese teachers.

None of the subjects thought of the present aspect of the experiment, although remarks were occasionally made by and to the college students to the effect that pictures of one sex or the other were easier to recognize, and mild disputes arose over the matter.

The results show that 4 of the 6 college students recognize members of their own sex better than members of the opposite sex. The same statement holds true for 5 of the 6 Japanese girls and for 4 of the 9 Japanese boys. An inspection of the

[1] From the Psychological Laboratory of the University of California.

tables (Tables I. and II.) shows that for college men and women of the same social status as the persons represented in the pictures it is easier for a subject to recognize a picture of one of his own sex than a picture of one of the opposite sex. The

TABLE I

COLLEGE STUDENTS

Subject	Recognized		Of a Possible Number for Each Sex
	Own Sex	Other Sex	
Miss Atkinson	151	133	190
" Collom	105	80	150
" Fisher	119	118	140
Total women	375	331	480
Mr. Hodgkin	169	147	180
" Clark	118	98	160
" McCoy	106	113	140
Total men	393	358	480

Total women's pictures recognized 733
Total men's " " 724

TABLE II

JAPANESE CHILDREN

Subject	Recognized		Of a Possible Number for Each Sex
	Own Sex	Other Sex	
Miss Fujiwi	172	142	210
" Takagi	134	131	180
" Ishibashi	180	163	220
" Morimoto	161	162	200
" Towata	149	139	200
" Sano	155	130	180
Total girls	951	867	1,190
Master Fukuzawa	170	159	210
" Tsuchiya	131	125	200
" Towata	114	101	170
" Miyanchi	114	110	200
" Yamagami	111	122	180
" Joseph	115	124	180
" Nakao	119	128	180
" Kiro	78	89	180
" Takeo	81	114	180
Total boys	1,033	1,072	1,680

Total women's pictures recognized 2023
Total men's " " 1900

pictures of neither sex possess any intrinsic advantage over those of the other; men's pictures are as easy to remember as women's in spite of the greater variety of dress and coiffure in the latter. But in the group of Japanese school children, who are of a different age and race from the persons represented in the pictures, the generalization does not hold. Women's pictures are more easily recognized by the girls, and also, though with many exceptions, by the boys. It may be noted that the work of this group of boys is distinctly inferior to that of the girls. On the surface of the figures, however, it appears that for these children there is an intrinsic advantage for the women's pictures, probably because of the greater variety of dress, etc., which overrides the possible preference of boys for men's pictures and is added to the actual preference of girls for women's pictures.

The characteristics of the pictures which led to recognition in this experiment have been carefully investigated through the observations of Miss Rose Wolf, a student with a good memory and thoroughly trained in psychological judgment. The observer accompanied each recognition with a statement of the basis on which it was made according to the captions of the following table (Table III.). No account was made of the failures to recognize nor of the cases of false recognition. It will be observed that this observer failed to recognize only 16 of the 380 pictures. All of these mistakes were made on pictures of men.

TABLE III

ANALYSIS OF RECOGNITION

	Men's Pictures	Women's Pictures
Decoration; clothing, manner of dressing the hair, etc.	46	102
Expression of face	82	40
Position; front, side, etc.	4	1
Resemblances to acquaintances	3	5
Expression and decoration combined	3	9
Position and expression combined	5	3
Decoration and position combined	1	0
Total	144	160
Total number of pictures shown	160	160

The table shows clearly that clothing, decoration and

facial expression are the most important criteria of recognition. Clothing and decoration help most in the case of women; facial expression in the case of men. As this observer is a marked example of the preference for pictures of one's own sex, the figures are capable of interpretation on the supposition that she was more familiar with the details of dress and ornamentation in her own sex and hence employed this criterion to greater advantage there. On this supposition it is evident that members of either sex will have an advantage with pictures of their own sex on account of the factor of dress and ornamentation, assuming that other factors remain constant.

ON CERTAIN FLUCTUATIONS IN CEREBRAL FUNCTION IN APHASICS

BY SHEPHERD IVORY FRANZ

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The results of the experiments recently reported by Brown and Sherrington[1] on the reversibility of action of certain motor centers in the cerebral cortex have again called attention to certain fluctuations in the activity of the cerebral mechanisms. These authors have shown that if at different times a so-called flexion point be stimulated the resultant activity may change, and instead of the normal flexion there may be produced an extension. Observations of somewhat similar reaction reversals have been made in diseases of the nervous system in man which, while not as anatomically well correlated as those of Brown and Sherrington on the monkey, have also some importance in the general consideration of the modes of cerebral activity. It might be hastily concluded that acceptance of these facts would force us to return to an earlier conception of the method of cerebral function, but it is perhaps more correct to conclude that the facts show the inadequacy of our present-day widely accepted views and of the exactness of our knowledge of the cerebral activities and of the cortical interrelations.

By experiments on *Rhesus* monkeys I have also shown that in different brains and in the two hemispheres of the same brain there are variations in the extent and in the location of the motor areas for the limb segments.[2] This fact has a direct bearing upon the question of variations in functional activity of the cerebrum, similar to that of the reversibility of action which has been described by Brown and Sher-

[1] Brown, T. G., and Sherrington, C. S., 'On the Instability of a Motor Point,' *Proceedings of the Royal Society*, 1912, B **85**, 250–277.

[2] Franz, S. I., 'On the Functions of the Cerebrum: Variations in Distribution of the Motor Centers,' *Psychological Monographs*, 1915, **19**, No. 1, 80–162.

rington, although it relates particularly to the variations in function of the similarly located collections of cells in the cerebral cortices of different animals.

In the course of several series of experiments with patients who were motor or sensory aphasics (using these convenient but misleading terms) variations in the daily capability to name certain objects, etc., were found. Since the observations indicate how fluctuating cerebral activity may be, they are here reported because they may be of interest in connection with other reports of variations in cerebral activity. Systematic observations have been made on four patients, who have been the subjects of an investigation of the rapidity, quantity, and character of speech reëducation.[1] All of the subjects showed variations such as will be described, but reports will be made at this time of only some of the variations in naming which have been shown by two of the subjects, G. and W. Summaries of the clinical histories of these two patients follow.

G., aged thirty-two, was a corporal in the Coast Artillery Corps. In 1907, after venereal infection he had intermittent headache and five years later confusion, with dulness and aphasia. He was unable to find words, but he understood simple orders. He was unable to write and did not talk voluntarily, and did not understand written questions. No paralysis of the vocal apparatus was discovered. The diagnosis was made of 'cerebral syphilis, tertiary, probably vascular, causing motor aphasia, agraphia, and partial apraxia.' The reason for the last (apraxia) is not given in the history. An examination two months later showed that some phrases and words could be said, such as the common 'yes, I know, but' in answer to questions, and in attempting to give the names of objects. He understood, as shown by his actions, some questions regarding himself and his family history, and he carried out simple acts at command, but not always. He was reported to be confused, but he played cards well, although he could not count and had to have another patient do the 'bidding' for him. Some slight

[1] A detailed account of the results of these experiments will be published later.

paresis of the face was suspected. Six months subsequently a clinical note was written to the effect that the patient had written a letter which was quite intelligible, but a few days later when he was asked to write out a simple request for parole he could not do so. This story is repeated several times in the history, as is likewise that of his ability to talk fairly freely on the wards with other patients and with the nurses, but not with the physicians. It was noted that 'his spontaneous speech is good, and he is able to make his wants known.' In three months' daily examination of the patient I was unable to confirm the statement regarding his ability to write, and that regarding his ability to talk freely. He was able, by the use of simple words, by gestures, and by a few expressions, to make his wants known, and to tell about himself to a limited extent, but his conversation could not be considered to be free. He made gross mistakes in copying the alphabet, he could write only a few of the letters voluntarily, and he could not write the names of objects which were presented to him. He could read aloud words and simple sentences well, but he could not repeat the names of some of the simplest and most commonly used objects. During the period of three months he did not on any occasion speak as freely as W., the other patient who is reported here. At no time did he give evidence of being confused. It is unfortunate that at least a copy of the letter, which we now know would have been a very important document, was not kept, and that accurate records of his supposed 'free' conversations with nurses were not made. It is possible, but lacking the documentary evidence very uncertain, that we may have had in his case some of the more marked variations in cerebral activity and cerebral ability of which minor ones are reported in this article.

W., aged thirty, poorly educated, an infantry private, became paralyzed about eight years ago, when he was serving in the Philippines. An Army surgeon certified that his paralysis and aphasia were due to 'a gumma in the left cerebral hemisphere,' but the reason for this conclusion is not apparent. The Wassermann reaction with the blood

serum has shown a 'trace' only once. Six years ago he could point out only a few objects which were named, but he understood simple questions. A clinical note written about a year later said that 'this patient is suffering from right hemiplegia with complete (*sic*) aphasia. He understands everything that is said to him; if his right hand was not paralyzed he could write his answers.' During the past five years it was repeatedly noted that he showed improvement in speech, but at the time the tests began there was a marked inability to name many objects, pictures, etc., which should have been familiar to him, he could not read the simplest words, and he could not write. Tests showed that he did not understand much of what was said to him, excepting simple sentences or clauses referring to his daily life and his wants. His spontaneous speech was made up of a number of names of objects which he saw almost daily, a favorite oath, and a few words indicating different activities, but he was able to repeat every name and simple phrase which was said to him. This patient appeared to be lazy or indifferent at times, but when his attention or interest appeared to wane he was stimulated to renewed activity. His efforts to learn were shown by his actions. He would repeat to himself a number of words in order to find the suitable name to speak, and he would clench his fist, wrinkle his brow, and show other evidences of mental effort.

In addition to teaching the names of objects of a familiar character the patients were also taught the names of ten ordinary colors (black, blue, brown, gray, green, pink, purple, red, white, and yellow) and ten shapes (circle, cross, diamond, heart, moon or crescent, oblong, oval, square, star, and triangle). The two series just mentioned were selected to obviate as far as possible special familiarity, although it was presumed that all subjects were familiar with the simple color names but not particularly familiar with the shape names. In the series with the different patients different procedures were used, but in all the name of each object, color, or shape, was given to the patient at least five times each day. In the object series with G. the names were given at least ten

times each day. Each object, or color, or shape, was 'named' by the patient ten times each day. In the following account the serial days, omitting Sundays and holidays, are mentioned and the total number of times a particular stimulus has been named for the subject can easily be calculated.

In the 'object' series with G. the fluctuations in naming are very apparent. Some of these are as follows: *Hinge:* For four days this name could not be given, on the fifth day it was correctly named seven times (out of a total of ten), but on the following day there was a decrease to three, subsequent to which the course of learning was regular and progressive with only slight variations. *Straw:* This name was given once on each of the first two days, five times on the third day, once on the fourth, twice on the fifth, three times on the sixth, not at all on the seventh, and thereafter the acquisition of the name was rapid and regular. *Button:* This was one of the most difficult names for the patient to acquire. For seventeen days the name was not given correctly once, although he had heard it at least 170 times. On the eighteenth and nineteenth days the name was given correctly three and four times respectively, but on the five succeeding days it was not given once. *Cannon:* On the first day this object was correctly named five times, on the second day not once, and on the third day it was correctly given seven times. The complete disappearance of the name for this object on the second day is the more remarkable in view of his previous occupation, namely that of an artilleryman. The variations in naming ability described for these four objects can be paralleled by a number of others, all of which show during one or more days a considerable degree of ability to name the object followed by a more or less complete loss of this ability, and a very quick return to the former level.

In the shape series with W. similar variations were noted. *Oblong*, which was named on the average more than five times in ten from the eighth to the thirteenth days, could be named only once on the following two days, and after a period of mediocre ability there was a complete inability

noted for the forty-fifth, forty-seventh and the forty-eighth days. Similarly with *square;* on the second say the name was given correctly five times in ten, but thereafter until the twenty-eighth day the name could scarcely be considered part of his usual vocabulary, since he used it correctly only twice on the fifth day, once on the ninth, and once each on the twenty-third, twenty-fourth, twenty-sixth, and twenty-seventh days. The naming of *triangle* also showed similar effects. Although difficult at first there was a gradual progression in learning until the seventeenth day, there being on the ten previous days an average of over five out of ten trials correct, with a maximum of nine on the sixteenth day. On the seventeenth day, however, the name could not be correctly given once in the ten trials, and on the following day it was given only once. During the succeeding eight days there was an increase in naming ability, average of over four in ten trials, which was followed by relative inability for two days, on each of which the name was used only once. On the following two days the name was correctly given nine times each. The subsequent period, although showing fluctuations, was one of gradual perfection. In this series also we have to note the fading away of a name for a period of one or more days followed by a return to a normal training level.

The 'color' series with W. also showed fluctuations worthy of note. *Black* on the third day and on the fourth day was named correctly ten times, on the fifth day, once, on the sixth day, seven times; on the seventh day, ten times. Here we find the almost complete dropping out of a name which appears to have been thoroughly learned. More remarkable, however, are the results for some of the later days. On the eighteenth day the name was correctly given five times, on the nineteenth day not once, on the twentieth day five times, and on the twenty-first day ten times. *Purple:* On the sixteenth day this name was given nine times; on the seventeenth day ten times, on the eighteenth day not once, on the nineteenth day five times and on the twentieth day seven times. The variations in the ability to name *blue* were even more marked. Some days it would range very high, to be

followed by a number of days with no apparent ability. The weekly percentages of correct responses (sixty tests each week) give the plainest indication of the course of the fluctuations. During the first week there was fourteen per cent. correct; in the second week, thirty per cent.; in the third week, twenty-three per cent.; in the fourth week, fifteen per cent.; fifth week, four per cent.; sixth week, nothing; seventh week, ten per cent.; thereafter a gradual increase in ability. These three examples show sufficiently the irregularity from zero to almost complete accuracy, or from almost complete mastery of the name to no ability.

The three series of tests show the same condition, namely, a variation from day to day in the ability to name certain objects, etc., instead of a gradual increase with minor variations such as is to be expected in a normal training series. At the same time these variations run to extremes of full accuracy to no ability, or the reverse. It is to be noted that these variations do not correspond by days, for there may be a loss on one day for one name and on another day when that name is suddenly picked up there is a loss for another name. Nor do the losses occur only once in a series with each object. In a few cases there is an apparent high ability followed by a loss of this ability, and after some days with a re-acquisition a second loss or great decrease. The case of 'black' in the series with W. is most instructive, because from his actions it was apparent that he knew the difference between that stimulus and the other stimuli in the series, but the name could not 'break through.' On certain days the only names he could give for this color were 'dark' and 'pitch dark,' although in the periods of training which accompanied the naming periods each day he showed an appreciation of his inability to get this name and frequently repeated to himself the word 'black' a number of times in order to get it fixed. For brief periods in different series other similarly obsessive-like names were given by all subjects, but not to as great a degree as in the case of W. with black.

Variations of the character described are probably similar to those difficulties in finding (or saying) the exact word

necessary for the expression of an idea which is sometimes experienced by normal people (assuming, of course, that not all of the naming difficulties are like those described by Freud and his followers). The inability of W. to give the name 'black' and his obsessive-like use of the terms 'dark' and 'pitch dark' are also like some experiences of normal people. The variations do not, I believe, resemble the 'attention' fluctuations which have been extensively studied by experimental psychologists, and which may have correlations with respiratory waves, for the subjects were allowed thirty seconds in which to give the name of a particular object, and in the 'color' series with W. when the wrong name was given he was told that it was wrong and he was permitted to give a second or a third or any number of names within the thirty-seconds period until he was satisfied that he had given the correct one. Nor are these results similar to the fluctuations which have been described by Küchler as being prominent in the naming ability of aphasics.[1] Küchler found that the addition of new objects and new words to a series interfered to a considerable degree with the percentages of correct answers for those things which had been in process of acquisition. This result is, however, only an example of exaggeration of the normal kind of disturbance in learning. In the 'color' and 'shape' series the same objects and the same number of objects were used each day, and the fluctuations which have been found can not, therefore, be due to newly added stimuli acting as interferences. In the 'object' series with G. the number of objects remained constant, but after the first six days when the name of an object had been learned a new object took the place of the old. Fluctuations due to the addition of new objects may have taken place, but in some of the examples which are noted above the great fluctuations occurred on days when no new stimuli were presented. At the same time it should be said that on the days when new objects were introduced into the series not only was there not always a decrease in ability to name the old, but in fact there was frequently a progressive increase.

[1] Küchler, H., 'Ein Fall von Wiedererlangung der Sprache nach neun Jahre alter Aphasie,' *Prag. med. Wochensch.*, 1893, **18**, 507–508; 520–522; 535–536.

The present results are more like the fluctuations in motor power which have been observed in paralyzed limbs by Sternberg,[1] phenomena which I have had opportunity to observe in cases of cerebral hemiplegia. Stertz[2] has also recorded considerable variations in the time of reactions and in other motor responses of patients with cerebral diseases. The interesting patient who has been studied by Stern[3] should also be mentioned in this connection for he showed not only fluctuations in motor power and coördination but also in the sensory fields. In the last mentioned case, however, the fluctuations appeared to come suddenly and they may have been of the nature of 'petit mal' attacks, although an epileptic condition was not obviously present. In the course of writing his name this patient would begin to lose some of his ability and the illustrations which are given by Stern show how rapidly the variations appeared. In connection with the fluctuations which I have found in aphasics mention may be made of an interesting observation of Brown-Séquard. This observation has also great interest in connection with the question of the possibilities of reëducation of aphasics as well as with the general subject of cerebral function. Brown-Séquard mentions fluctuations in the ability of aphasics to speak and records as a fact that 'there are cases of aphasia where the diseased person has had the power of speech restored during delirium.'[4]

The results obtained with the aphasic patients cannot be

[1] Sternberg, M., 'Ueber die Kraft der Hemiplegiker,' *Dtsch. Zsch. f. Nervenhk.*, 1908, **34**, 128–153.

[2] Stertz, G., 'Ueber periodische Schwankungen der Hirnfunktion,' *Arch. f. Psychiatr. u. Nervenkr.*, 1911, **44**, 199–155.

[3] Stern, R., 'Ueber periodische Schwankungen der Hirnrindenfunktion,' *Arch. f. Psychiatr. u. Nervenkr.*, 1895, **27**, 850–917.

[4] Quoted in the *London Medical Record*, 1874, **2**, 334–335, from a summary of a lecture published in *Nature*, from advanced sheets of the report in the *New York Tribune*. I have been unable to find a fuller account of this interesting lecture, unless a brief article published ten years later is the one which contains the facts referred to (Brown-Séquard, 'Persistance de la parole, dans le chant, dans les rêves et dans le delire, chez des aphasiques,' *Comptes Rendus de la Soc. de Biol.*, 1884, 8 sér., **1**, 256–257). In that article the author mentions a case of motor aphasia in which there was a considerable return of speech during delirium, the patient using words to describe his hallucinations during that added abnormal condition.

satisfactorily explained by themselves, but in connection with other experimental and clinical facts they indicate a degree of variability of cerebral functioning beyond that which we usually consider may be present.[1] Several explanations of the condition suggest themselves, the more probable being that we have in these cases examples of the variations in permeability of neuronic connections or in the degree of smoothening out of paths. That we may be dealing with a cerebral condition akin to that diseased state known by the French as 'intermittent claudication' is a possibility, but in view of our lack of knowledge regarding localized variations in blood supply in the cerebrum the explanation cannot be seriously considered at the present time. Whether or not the facts can be satisfactorily explained they point to a less exclusively machine-like view of the method of cerebral activities and in this way they lead to a better understanding of the activities of collections of cerebral cells which are commonly called centers.

[1] We cannot, of course, exclude the so-called lower centers when we deal with motor phenomena, or with behavior of the type under discussion.